History of Science Library

The Road to Medical Enlightenment 1650–1695

by Lester S. King

THE ROAD TO MEDICAL ENLIGHTENMENT 1650–1695

In the second part of the seventeenth century a whole series of brilliant scientists, born into what was already an atmosphere of innovation, came to the height of their powers. Under attack was the prevailing Galenism, which rested on the Aristotelian philosophy and had reigned supreme since the second century A.D. Many new systems were being put forward. Dr. King selects for special study the ideas of five men: Riverius still clung to Galenism while others rejected it; van Helmont leant on the neoplatonism of Paracelsus; the famous English scientist, Robert Boyle, rested his medical theories on the 'new' atomism. In contrast, the rationalist Sylvius sought a basis for medicine in chemistry rather than in metaphysics, and the empiricist Thomas Sydenham, similarly spurning metaphysics, claimed that his theories derived directly and solely from what was observable.

Dr. King goes on to discuss more briefly a number of other figures whose various ideas on medicine show in aggregate how shifting and uncertain were the patterns that ran through the intellectual life of the era. He then deals at length with Friedrich Hoffmann, whose *Fundamenta Medicina*, published in 1695, is perhaps the best single expression of the new medicine. In it may be seen the beginnings of the synthesis and development of seventeenth-century ideas which mark the true dawn of the medical enlightenment.

This is an elegantly written and absorbing book. Dr. King describes with great clarity, and with sympathy, the ideas of the various men with whom he deals. His book gives fascinating insights into the intellectual qualities of men who, even if their methods appear by today's standards inadequate, were in the context of their own times thinkers and practitioners of outstanding stature.

Dr. King is senior editor of the *Journal of the American Medical Association* and professorial lecturer in the History of Medicine at the University of Chicago.

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1650–1695

LESTER S. KING

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Preface

In the past dozen or so years, while studying the doctrines of eighteenth-century physicians, I became particularly interested in the writings of Friederich Hoffmann. Like Boerhaave and Stahl, he reached full maturity in the eighteenth century and exerted his maximum influence during this period. But these men were educated in the seventeenth century and presumably reflected its thought modes.

The terms 'seventeenth century' and 'eighteenth century' flow easily down the scribbling pen, as if they were substantial in their own right. A division into centuries is quite useful in ordering the time stream, but the flow of events and their interrelationships do not ordinarily fall into compartments corresponding to the decimal system. The terw 'eighteenth century' has a very rich meaning, full of overtones. Hom meaningfully can we circumscribe it by a term of years? The calendar does indeed offer one way of defining its boundaries. Perhaps we can find a better measure in the events themselves and their interconnections, rather than in any particular number of orbits around the sun.

The historian, looking at events, seeks relationships and connections within the total welter. He must identify events that somehow seem to belong together, and he must draw them into some sort of unity. He may use various terms—a revolution, a rebirth, a period of darkness, or a period of illumination, or perhaps a transition from one to another. Such terms as 'Dark Ages', 'Renaissance', 'Enlightenment', 'Scientific Revolution' are shorthand expressions, each serving to identify clusters of events, particular segments of the time stream. But the togetherness which brings events into a cluster and provides them with a unity depends not so much on the events themselves as on the perception of individual historians. Other historians may perceive new clustering and completely change the nomenclature, provide new labels. And the river of events will patiently accept that identification.

Within any artificial time span, such as a hundred years, we can

perceive many different trends and movements in different stages of growth, maturity, or decay. In the hundred years beginning January 1, 1701, one dominant trend has become known as rationalism, which flourished mightily during this century. But of course—if we can continue the metaphor of a stream of events—we can say that the origins of rationalism must be sought far 'upstream', while its effects, after the main turbulence had subsided, continued far 'downstream', even to the present day.

What were some of the 'upstream' factors which contributed to the full development of rationalism in medicine? The question became particularly significant to me when, in my studies on Friederich Hoffmann, I read his *Fundamenta Medicinae*, written in 1695. This early work, more than his later and better-known writings, shows rationalism 'in the making'. It developed out of the conflicting currents and cross-currents of an earlier period.

In the present book I try to analyze historical data and provide a certain amount of order for a confusing rush of events. But the order that I find reflects my own perceptions, analysis, and interpretation. I have tried to make these convincing, that is, to make my readers see things as I do, without in any way implying that there are no other ways of interpreting the same events, no other clusters into which they may be gathered.

It is a pleasure to express my thanks to Professor Donald Lach of the University of Chicago, who helped me select a suitable illustration for the book jacket. And my indebtedness to my secretary, Mrs. Merry Uchida, is immeasurable. In a very literal sense her patience and skill made this book possible.

Lester S. King,
Chicago,
October, 1969

Chapter One

Introduction

'REVOLUTION' AND 'ENLIGHTENMENT'

Galen died in 200 A.D., and for the next thirteen centuries his doctrines, in a well integrated system, dominated medical thought without serious competition. Galenism—the aggregate of his teachings—is a term not easy to define. If we try to sum up the essence in a few words we can characterize it as a systematic and carefully wrought doctrine that embodied the metaphysics of Aristotle, and accorded prime place to 'forms' and qualities; that exhibited the concepts of dynamism and teleology; that showed careful reasoning and also abundant empirical observation; that had an answer for every question; and that harmonized with the teachings of the Church. Some of the specific medical teachings we will discuss in the next chapter. Suffice here to indicate the harmony of Galenism with the intellectual currents of the time and the intellectual needs of the educated people.

By the 16th century, however, significant dissension appeared. The Aristotelian philosophy was under attack, the Church became divided, the intellectual and cultural needs of the people changed, and Galenism suffered along with all the other modalities. In the 17th century Galenic teaching came under concerted attack, and by the end of the 18th century Galenism as a system had entirely disintegrated. The persistence of Galenic medicine for such a long period, and its relatively rapid decline and fall, form a remarkable phenomenon in intellectual history.

This decline, of course, was not an isolated phenomenon. The 15th and 16th centuries saw tremendous changes in western Europe in all phases of human endeavour—politics, economics, religion, philosophy,

science, art. As men became more adventuresome, intellectually and physically, the old order changed and yielded place to new.

It has long been fashionable to call this change the Renaissance. At one time historians thought that it all happened quite suddenly, but historical evaluations, like all other phenomena, also undergo change. The more the period was studied, the more complex it seemed to get and the more tangled its roots. The different cultural and intellectual components changed at different rates, comparable, perhaps, to the way various parts of the body grow at different rates during puberty. Medicine, in its broad sense, was merely one phase in a total pattern, and the crumbling of Galenism was part of the transformation to what we call the modern world.

This transition to the modern world has been studded with so-called 'revolutions'—in politics, economics, philosophy, religion, art, science, medicine. The Industrial Revolution and the Scientific Revolution are especially well accepted terms that indicate, in these areas, a destruction of the old dominant modes and a development of the new. By analogy, what could be more appropriate than the concept of 'Medical Revolution' to indicate the fall of Galenism, and the coming-into-being of new medical concepts, attitudes, techniques? The idea deserves detailed examination.

To indicate the changes that have taken place we might embark on a jet-propelled survey of medicine. When we cover the period from Galen to modern medicine, we find certain clusterings of important figures. As we enter the early 17th century we find Harvey's discovery of the circulation the outstanding medical contribution, but Harvey was only one of the geniuses that profoundly influenced medicine, directly or indirectly. In medical history he must share the spotlight with such non-medical figures as Bacon, Galileo, and Descartes. These men influenced all of scientific progress. They acted as discoverers, so to speak, in the new world of science.

A whole series of great men, born in the first half of the 17th century and brought up in the tradition of the great innovators, reached the height of their power and influence in the second half of the century. Men like Borelli (1608–1679), Sydenham (1624–1689), Malpighi (1628–1694), Willis (1621–1675), Boyle (1627–1691), Leeuwenhoek (1632–1727), Locke (1632–1704), to mention but a few, come immediately to mind. In the new intellectual world of science they were, so to speak, the first generation of settlers. Newton (1642–1727) and Leibniz

(1646–1716) were in a class by themselves, but actually had little influence on medical progress at this time.

The men who acted as medical leaders in the early 18th century were born in the last half of the 17th century. Particularly significant were Baglivi (1660–1704), Hoffmann (1660–1742), Stahl (1660–1734), and above all, Boerhaave (1668–1738). The latter dominated medical theory for the first half of the 18th century, and for about twenty-five years, from 1715 to 1740, his system of medicine was virtually supreme. He had synthesized into a satisfactory whole the various strands derived from the new mechanical philosophy, the new chemistry, and the remnants of classical Galenism.

Beginning about 1740 a vigorous experimental and empirical approach struggled with the dogmatic systems of the earlier part of the century. Dogmatism remained, indeed, but new discoveries, new observations in experimental biology, embryology, physiology, pathology, and clinical medicine, made great advances. To name but a few of the leaders, we can mention Trembley, Haller, Wolff, Cullen, Morgagni, Sauvages, John Hunter. Progress continued along several fronts with substantial changes gradually taking place in the medical sciences.

The development of an enlightened empiricism, with more attention to the whole patient, led to a revised vitalistic approach and to new insights. At the same time the growth of chemical theory and practice subjected medical theory to new strains. Bichat (1771–1802), who died at the turn of the 18th century, marked the end of one era and the beginning of another. By this time Galenism was indeed dead, but the new replacement had not yet taken full shape.

While the 18th century might have seemed like a long arid period in medicine, actually it was the gestation period, from which was eventually delivered the flourishing progress of the mid-19th century. After the turn of the century there occurred tremendous and rapid developments in physiology, chemistry, microscopic anatomy and pathology, the so-called basic sciences became firmly established as fundamental to medical progress, and at the same time clinical medicine made rapid strides. Clinicians showed new powers of discrimination and logical thought, held new ideas of the validity of evidence and inference. By mid-19th century 'modern' medicine was well established.

New advances succeeded each other with startling rapidity, and the very rate of change, the rate of progress, showed a striking

increase. Anaesthesia, the germ theory of disease, the discoveries in microbiology, development of antisepsis and asepsis, continued progress in chemistry, pharmacology, physiology, pathology, microscopy, the discovery of new techniques such as x-rays, the new disciplines such as immunology or genetics, and above all, the current adaptation of electronics in the service of medicine—all have produced a transformation that almost beggars description.

When we look back at Galenic medicine we have the feeling that we have indeed undergone a profound revolution. This is an easy epithet to utter. But if we return from jet propulsion to a more modest speed, and regard the changes in medicine with a more critical eye, we find some disturbing factors in regard to the concept of 'revolution'.

We can, if we want, find a surprising number of 'revolutions'. If we compare present-day medicine with that of only thirty years ago, the progression might certainly merit the epithet 'revolutionary'. New diagnostic means and new techniques of treatment render the medicine of even thirty years ago quite obsolete. If, however, we turn back merely one more generation, we find an enormous difference between, say, 1935 and the Oslerian medicine at the turn of the century. And Osle rhimself, of course, lived through the revolution that microbiology and asepsis brought about in the last third of the 19th century. Then, we find a prominent historian referring to the 1830's and 1840's as follows:

> 'An entirely new clinical medicine developed during the first part of the nineteenth century . . . the violence with which clinical medicine was completely revolutionized in the course of a few decades . . . will remain one of the wonders of medical history.'[1]

And a physician, writing in 1841, declared that

> 'Never before . . . has it [medicine] made such rapid and sure progress as for the last 40 years.'[2]

Since the past 160 years represent the full flowering of medical progress, we are not surprised at the many and rapid advances. Nor are we surprised that during this period physicians themselves appreciated the extent of the progress. However, even the 17th century physicians were fully aware of revolutionary progress. Nedham, in

1665, commented on the tremendous changes in medicine of the preceding fifty or sixty years and noted that in the past twenty years no other science had ever made such an advance.[3]

Change is always present but its rate is variable. Do we need any minimum velocity before we can call a change a revolution? Or any minimum extent? In politics the term connotes something sudden and violent. Yet the more closely we examine what purports to be sudden, the more precursors do we find. And in non-political fields, like science, we find neither violence nor suddenness. The only criterion we have is the intensity of the change. 'Revolution' is an acceptable word for change of such degree that what was formerly dominant becomes subordinate or disappears, while other manifestations, hitherto subordinate or latent, become dominant.

By appropriate selection we can discover as many 'revolutions' as we wish. On the other hand, we can reduce the number by requiring a greater profundity of change. A recent British writer,[4] surveying all of medical history from early times to the present, had a very lofty perspective and found but three revolutions. The first occurred when the primitive animistic explanations of disease (with the treatment appropriate thereto) gave way to the naturalistic attitude exemplified by Hippocrates. The second began in the 16th century with the realization that accurate measurement and experimental method were relevant to biology and medicine. Vesalius and Harvey were offered as exemplars. The third revolution started in the 19th century and, still in progress, involves the relationship of medicine with society.

Thomson represents one extreme which, in over 2,500 years, could find only three periods of change sufficiently profound to be called revolutionary. At the opposite extreme we can, if we look resolutely enough, find a revolution every thirty years or so.

In medicine the difficulties that we encounter in trying to understand the term 'revolution' have become intensified with Kuhn's recent book,[5] in which he tried to analyse the common factors in successive scientific revolutions. Unfortunately, he has tied himself down with a rather lamentable terminology centering around key terms of 'paradigm' and 'normal science'. The 'paradigm' is apparently the conceptual formulation designed to explain certain phenomena, and generally accepted by the leaders in a field. 'Normal science' is the activity which shows that a particular set of concepts does in fact apply

to a wide range of phenomena. In a sense, the paradigm is a box and the normal science is the activity whereby scientists fill up the box and find that the contents fit. But sooner or later anomalies appear and some phenomenon does not 'fit'. Where a scientist had expected one result to occur he actually found a different result. 'Anomaly appears only against the background provided by the paradigm.'[6] Anomalies may lead to a new paradigm. A revolution occurs when a paradigm is partly or wholly replaced by 'an incompatible new one'.[7] That is, the scientist gives up the original conceptual formulation and adopts a new one, suitable to explaining the new anomalies as well as the older data, but incompatible with the previous formulation.

Kuhn is approaching a major problem in the history of ideas—How do new concepts replace old ones? Unfortunately, his rigid and not entirely happy terminology entails qualifications and sub-qualifications that impair its application. He has introduced such complications as the pre-paradigm stages, 'mature' and 'immature' sciences, and the notion of small revolutions and large revolutions, and other limiting features.

Whether his formulation has sound application to the physical sciences is outside the scope of this discussion. But Kuhn apparently realized that his schema, enunciated for the physical sciences, did not apply to medicine. Hence he merely eliminated medicine from the sciences. He considered medicine a craft and bracketed it with calendar-making and metallurgy.[8]

We can criticize Kuhn's evaluation of medicine as quite unhistorical, for it ignored the views held by many older physicians who felt that their medical doctrines *did* constitute a science. The historian should not ignore the evaluations current in earlier eras, and impose 20th century values on unwilling data. But 'presentism' of this character is only a minor point. Far more important than the elimination of medicine from the sciences is the irrelevance of Kuhn's formulation to the real problem at hand—namely, how does it happen that new concepts succeed old ones? Medicine had its concepts, and the concepts changed. How? Why? These are the important problems. Whether ideas that at any given time dominated medical thinking should be called a 'paradigm' or a 'pre-paradigm' or an immature science, is of little importance. The historian of medicine must first of all identify basic and underlying concepts, and if possible trace their changes and try to account for the changes. The concepts, often contradictory, were of

surprisingly broad extent. They involved factors that frequently conflicted, in areas as diverse as metaphysics, physics, logical methodology, and sociology, as well as medicine in the strict sense. Kuhn's formulation simply does not take into account the interpenetration of concepts relevant to medicine.

II

In histories of medicine the specifically medical discoveries receive the most attention. New findings, for example, in anatomy or physiology, or the identification of new diseases, or the introduction of new therapeutic agents, are ordinarily well covered. But nevertheless these represent but a small part of medical history in its larger scene.

If we probe further relations, we see first of all that medicine has close connections with various sciences, particularly chemistry and physics. These 'hard' sciences provide technical help, ranging from simple tests for urine examination to telemetry electron microscopy. But of far more consequence, science offers theoretical and conceptual aids, and medical theory has always drawn heavily on theories prevalent in other sciences, including biology. The 'first principles' of medicine in large part stemmed directly from the principles that dominated other branches of scientific studies. And as these sciences changed their principles, so too did medical theories.

But we must go much further afield. If we explore the wider confines we note that medical practitioners are members of society and share in all the problems that face any social unit. The effect of social pressures on physicians as individuals, and on medicine as a profession, form a significant part of medical history; and equally important are the reciprocal relations—the effects of medical doctrine and medical practice on society. The sociology of medicine is only just beginning to receive appropriate attention. Equally significant are other interactions of medicine—with economic trends or educational practices or political or military activity.

When we study interrelations such as these, we realize that medicine cannot be regarded as a self-contained discipline. It must be considered part of a general cultural milieu, within which some modalities are

more important than others. One link of particular significance is that between medicine and philosophy.

The word 'philosophy' can mean all things to all men and has no precise definition that commands general assent. At one time philosophy comprised the domain of all knowledge, but gradually had to give up one area after another to more specialized disciplines that declared their independence. 'Philosophy of nature', for example, became 'natural philosophy', which in turn became 'science'—unqualified. The study of the mind (and, to some extent, the soul) turned into psychology. Ethics and aesthetics remained partly in the camp of philosophy, but in part moved over into the 'liberated' domain called science. However, the study of ultimate first principles—metaphysics—has always remained a philosophic discipline. Unfortunately, the intimate relations of metaphysics to medicine has received inadequate attention.

The ultimate first principles that men hold determine their attitudes toward the universe, decide what they consider important, and provide, so to speak, the spectacles through which they see the world. These conceptual spectacles determine, for example, what men believe about the nature of God, the relations of God and man and the world, the question of good and evil and moral values. History can study the way these basic questions impinged on the daily life and the daily activities of the common man, or on church doctrines, or on various establishments. The different and often conflicting answers depended on the different philosophic views, which formed part of the general cultural milieu.

Metaphysical views interact with cultural and educational factors and with the individual personality factors to produce patterns of reaction. These determine not only cosmic attitudes, but, more practically, the values and attitudes on a very concrete level. What is it that men will actually look at? What will they see and what will they ignore? What comes within the range of their vision and what falls, so to speak, on the intellectual blind spot? And of what they do see, what will they consider important?

These questions pertain to what I call the frame of reference. Let me illustrate by a rather extended metaphor. Imagine a young stamp collector who has a large collection of miscellaneous stamps that he wishes to place in some sort of order. Now, from the 'obvious' or 'logical' standpoint (which is generally synonymous with what we

ourselves happen to believe) the 'proper' way to arrange the stamps is to separate them first by the country of issue, and then by date of issue in chronological order. Once we achieve such an elementary classification, then any new stamps—i.e., any new 'data'—could readily be fitted in.

But there are other ways of bringing order into the crude data. We could, for example, pay attention to the pictures on the stamps, and classify them into categories of animals, plants, people, places, and the like. Then we could find further subdivisions. Thus, under animals, we could divide into birds, mammals, fish, and so on. People we could subdivide according to their activities—scientists, writers, artists, and so on.

Now these two schemes of classification, so very different one from the other, are equally valid. Each of them bestows order on a mass of material initially without order. The particular scheme of classification that we adopt will direct our attention to certain features. And in so doing it *automatically causes us to ignore other features as irrelevant.* From one point of view it is more important to know whether a stamp shows an elephant or kiwi, than it is to know whether it originated in Zanzibar or Canada. But from a different point of view this is not the case. Antecedent interests and concepts thus govern the way we look at things, and determine what we consider important and what we ignore.

In this example, the classification we adopt, causing us to look for certain things and to ignore others, reflects basic attitudes. These determine not only what we look at but how we evaluate what we see. If we change our basic attitude we have a totally different evaluation. A simple example we can find in a pair of well-known proverbs. The expression 'Many hands make light work' depicts a certain situation with an implicit scheme of values. However, if we re-examine the situation with a different viewpoint, we might use the proverb 'Too many cooks spoil the broth', which immediately involves a quite different set of values. The choice would depend on the way we regard the total situation, rather than on the situation itself. Similarly, we can reflect on the choice between 'The glass is half full' and 'The glass is half empty'. Or, we may think of the optical illusion, wherein we can make the 'staircase' come toward us or recede from us by a shift of mental set, or attitude. The mental set, or attitude, reflects philosophic and cultural influences as well as personal idiosyncrasies.

All this has sharp relevance to medicine. For example, physicians have always dealt with sick people and concerned themselves with establishing diagnosis, with finding the cause of disease, with instituting treatment appropriate for the diagnosis and the cause. To accomplish all this the medical practitioner must make observations, evaluate these observations, make the various inferences and act upon them. In addition to acting in concrete situations, the physician must build a theoretical superstructure which can satisfy the intellect.

In all this medical activity the physician is *looking at the world in a particular fashion*. If we keep this in mind we can have a deeper appreciation of medical doctrines now long outmoded, and a better understanding of doctrines that are obviously 'wrong'. We realize that the way the physician looks at the world determines what he will see, what he accepts, what he rejects.

What, for example, does he accept as 'evidence'? What does he accept as a 'fact', to serve as the basis for logical inference? How much reliance does he place on perceptual data, and how much evidence does he require to ensure reliability? How cogent for him is the single observation or the single experiment? How much conviction should it carry? What phenomena require explanation? What is he willing to ignore? Since he cannot explain everything, he must necessarily attend to those aspects that he wants to explain and sweep the residue under the rug as not important. These are a few of the topics or questions whose answers depend on our basic attitudes. These, in turn, depend in large part on our philosophic presuppositions.

There is a related series of problems that revolve around the question of critical acumen, as in the simple question, 'How do you know?' The historian must ask, what is it that carries conviction? How valid are the statements of authority? On what evidence has authority maintained itself?

Different world-views have affected physicians and determined their way of looking at things, have provided different climates of credulity, different levels of critical awareness. The great philosophers of the past, Plato and Democritus and Aristotle, Plotinus, Bacon, and Descartes, have influenced values and attitudes of physicians, no less than of theologians, And philosophers, through their influence on the cultural milieu, have been intimately concerned with the development of medical ideas, just as intimately as have specifically anatomical discoveries.

III

What is generally called the Enlightenment, in its historical sense, was an intellectual movement of the 17th and 18th centuries, which centred on the right use of reason for acquiring knowledge and achieving happiness. It embraced the newer concepts of science and the philosophy behind them, criticized the traditional religious views, forged new principles of methodology, and manifested itself in philosophy, religion, ethics, politics, economics, literature, as well as in science, all in an interrelated whole. The Enlightenment, as a specific historical movement, became differentiated in the second half of the 17th century and reached more or less of a terminus around the end of the 18th century.

Temporal subdivisions are quite arbitrary, but for orientation purposes, I like the subdivisions and nomenclature offered by Trevor-Roper.[9] Dealing with the 300 years from 1500 to 1800, he considered 1500 to 1620 as the 'Age of the European Renaissance'; 1620 to 1660 as the 'Period of Revolution'; and the interval 1660 to 1800 as the 'Age of the Enlightenment.' The actual terms, which are conventional, do not particularly concern us, but the time intervals are quite significant for medicine and medical progress. The period 1620–1660, that Trevor-Roper designates as 'revolutionary', witnessed the ideas of Harvey in physiology and the major writings of Francis Bacon and Descartes, as well as other important figures destined to have important effects in medicine. The year 1660 is a useful landmark to indicate the approximate beginning of important changes in medicine, as well as in the other modalities mentioned.

The Enlightenment, in its traditional sense, is a well-defined historical movement. But we can also construe the word more simply and naively, namely, as *enabling us to see things in a clearer light*. By 1800, or better yet, 1830 or so, physicians looked at the world quite differently than in 1660. The term 'enlightenment', as I use it here—and I use it with a lower case 'e'—indicates the new and clearer way of looking at the world, and implies that the attitudes towards things have changed more than the things themselves. It indicates a new critical acumen, a new regard for empiricism, a new approach to evidence and new concepts of validity. Bichat, in 1800, had tools to work with not too

different from those that Friedrich Hoffmann had in 1700, but the mental set and attitudes and critical awareness were vastly different. New facts, of course, had been discovered. But even more important was the altered way of regarding the facts. *They were looked at in a different light.* This is a sound usage of the term 'enlightenment'.

As I use this word, it involves a long slow process with continued interaction of different aspects, a feedback with mutual reinforcement in some areas, antagonism in others. It implies process and continuity and not any sudden break; it implies complex overtones and not any simplistic scheme. It involves nothing less than the entire intellectual history of the period. The word 'revolution', on the other hand, has quite different implications and is much less satisfactory for analysing the trends in medical history in the past 400 years.

How did the change come about? As I regard the period from approximately 1650 or 1660 to 1830 or thereabouts, I perceive a continuous stream, but one which underwent several changes in direction. The interval from about 1650 to approximately the end of the century showed an early struggle for dominance among several competing currents. From about 1700 to 1740 or so there was a pseudo-clarification, a sort of 'false dawn', as doctrines became systematized. We might speak of it even as a period of consolidation and rigidity. From about 1740 to 1800 occurred a new spurt, with a new empiricism and a substantial decline of the earlier currents; and from 1800 to about 1830 there was remarkable progress, as the changes became intensified and the empirical stream picked up momentum. At no time did the course progress uniformly, but constantly there were swirls and eddies, backwaters and counter-currents which preserved many earlier characteristics. Yet unquestionably the mainstream progressed.

I suggest, rather arbitrarily, that the 'enlightenment' begins about 1700, and the latter part of the 17th century may best be interpreted as merely the approach thereto. During the latter part of the 17th century, as I view it, several different trends were active, and no clear dominance emerged until almost the very end of the century. In the early 18th century 'rationalism' had become predominant and, serving as the first real stage of the enlightenment, itself underwent considerable later change. To use another figure of speech, we can say that the latter 17th century showed several distinct threads, and only when these got woven together did the enlightenment properly begin. But I have no quarrel with historians who do not wish to accept this termin-

ology. My chief interest is to examine some of the threads that were prominent in the latter 17th century and that entered into the 18th century fabric.

In no sense does this book offer a history of 17th century medicine. It is deliberately episodic, focusing on certain key figures who exemplify, in medicine, particular aspects of intellectual history. I keep in mind especially such aspects as the metaphysical background, methodological viewpoints, and the degree of critical acumen. I begin with the prevalent Galenic viewpoint (and its Aristotelian metaphysics) and choose as its exemplar Lazar Riverius, with his carefully articulated system. Two other competing metaphysical viewpoints were the neoplatonic and the atomistic, and for these I discuss van Helmont and Robert Boyle, respectively. The doctrines of these men depend quite directly on the metaphysical foundation. But other workers were concerned more with methodology than with metaphysics, and as examples I choose Sylvius as an iatrochemist, and Sydenham as an empirically oriented clinician. A number of minor figures reveal various degrees or critical acumen, and several of these men are briefly discussed in one chapter. By the end of the century a synthesis began to occur and the best example of this is a little known work, of 1695, written by Friedrich Hoffmann, and embodying a new and 'modern' system.

Those who want to study 17th century medicine find difficulty in getting hold of primary source material in English. Many important authors wrote in Latin and their works have either never been translated, or have appeared in contemporary translations that are far from satisfactory, and in any case very rare. One goal of this book is to expound the medical teachings of the various men discussed. In the various chapters I have presented the salient and substantive doctrinal features, so far as they are relevant to the issues at hand, but with no attempt at a complete exposition. I try to enter sympathetically into the teachings of each man in turn, and to see things through his eyes, to understand the premises on which he built, but always within the deliberately assumed limits of this book.

Quite obviously I might have chosen quite differently. Just as important as the men I have selected would be others such as Descartes, Sennert, Borelli, Willis, Lower, John Locke, Baglivi, to name but a few. But I did not choose these. I have tried to indicate a development in modern medicine by discussing a few particular individuals, without in any way denying that other men are equally important.

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Chapter Two

Metaphysical Positions

RIVERIUS

Galenic medicine had satisfied the best medical minds for almost 1500 years. To study its decline in the 17th century we must first understand some of its major features. We realize, of course, that the Master's teachings did not maintain their original purity in the centuries following his death in 200 A.D. The transmission of Galenic thought to the Arabian world, and then back to the western world, naturally introduced certain changes. Furthermore, various physicians of the 16th and 17th centuries might all consider themselves Galenists and yet differ substantially among themselves. However, it is not my purpose to discuss fine differences between the original Galenic writings and those of far distant disciples, many centuries removed, nor to compare various latter-day Galenists one with another.

The Galenic teachings in their broad outline provided a set of conceptual spectacles through which physicians examined the world of health and disease. To indicate the point of view which these spectacles provided I choose the writings of Lazar Riverius.

Riverius was intimately connected with the city of Montpellier, and with its famous medical school. Born in Montpellier in 1589, he spent virtually all his life there. He secured his M.D. degree from the University in 1611, and engaged in a lifetime of teaching and practice. He acquired a great reputation as a practitioner and teacher, was appointed Professor of Practical Medicine in 1622, and refused calls to other universities. Although he was a Galenist he did have 'modern' leanings in his therapy and he was the first to introduce into Montpellier the spagyric medications such as pure metallic substances.

However, he avoided the Paracelsian extravagances which were a source of prejudice. His principal works consist of the *Praxis Medica*,[1] first published in Paris in 1640; the *Methodus Curandarum Febrium*, that appeared in Paris in 1648; and the *Institutiones Medicae*, published in Leipzig in 1655. His collected works were first brought out in 1663 and there were many subsequent editions—as late as 1723, 1728, 1735, and 1738.

His works fall into two categories, the practical and the theoretical. The practical medicine we see clearly in his *Praxis Medica*, but perhaps even better in his 'Centuries' of case reports, the descriptions of 'noteworthy' medical observations and cures.[2] These furnish an invaluable storehouse of interesting material on nosology, diagnosis, therapy, processes of reasoning, and clinical acumen. The presentations are non-systematic but nonetheless valuable. While today we have a natural tendency to examine these with present-day diagnosis in mind, such an approach has little significance or value. Far more important, we can see how a physician in the 17th century approached the diagnosis and treatment of a patient. In these case reports much medical theory is hidden away, expressed more by implication than by direct exposition.

The second category of Riverius' writings is the theoretical.[3] He provided an excellent resumé of the basic principles on which his medical doctrine rested, expounded in orderly and lucid fashion and in relatively brief compass. The principles are essentially Galenic. His contemporaries, who also would be called Galenists, might offer somewhat different interpretations, but these differences would be irrelevant to our purposes.

The chronology has particular significance. The *Institutiones Medicae* was first published in 1655, but the new physiology, such as the circulation of the blood, had not yet found a place therein. Newer ideas, such as the mechanical philosophy, were not mentioned, even though Descartes had died in 1650 and Gassendi in 1654. In contrast to Riverius, Sennert, an equally staunch Galenist, did have some stirrings toward atomism.[4]

We are not particularly surprised that the new ideas had not found wide acceptance in 1655, for a strongly conservative bent permeated the 17th century. Far more surprising, however, is the large number of reprintings that Riverius' work underwent. Editions published through the 17th century and well into the 18th century maintained the original

formulation without significant change. It is quite startling to study a medical imprint of 1728 which, one hundred years after Harvey's publication, still presents the Galenic ideas of the heart.

Nevertheless, so highly was he esteemed as a practitioner that long after his death a book was published, *Riverius Reformatus or the Modern Riverius*,[5] that enjoyed a vigorous popularity. This book, as its title indicates, tried to bring Riverius' practice into harmony with the teachings of the early 18th century.

To appreciate Riverius we must understand his belief that medicine is a science. This may have a hollow sound today, for to us the Galenic concepts seem the exact reverse. Today 'science' brings up a totally different image—controlled experiments with formidable apparatus and gadgets, inductive generalization, hypothesis and verification, and practical benefits. We have all been strongly influenced by Francis Bacon who so heartily condemned Aristotle, scholastic methodology, and the spinning of fine distinctions. Bacon's superb rhetoric has left a powerful if not altogether fortunate impress on our minds, so that Galenic methodology, in its theoretical aspects, seems anti-science by today's views. But this is error.

Riverius considered science as a body of knowledge resting on first principles that are true and established, and from which all particulars can be derived by deduction. Science deals with first principles, generalizations, and their logical derivation and interrelation. The essence of science is *knowing* and not doing.[6] *Doing*—the activity directed to practical ends and particular cases—was art and not science. This doctrine, of course, rests on Aristotle's analysis.[7]

Riverius indicated that medicine must be considered in a two-fold aspect. First, it is taught 'from principles that are certain, firm, and unquestionable' (*certis, firmis, & indubitatis principiis*).[8] Medicine is to be considered a science like astrology [astronomy], geometry, or arithmetic, considered as purely conceptual, as bodies of knowledge. On the other hand, these subjects may also have practical application: astronomy in the telling of accurate time, geometry in measuring land, or arithmetic in actual computation. And in this viewpoint medicine has also a practical end, the safeguarding of health, if present, or the restoration of health, if lost. Thus medicine was both a science and an art. Then, as now, the physician must treat particular patients as

individuals; he must also know the principles of science (today we would call them the 'basic sciences'). The physician lives in two worlds, one theoretical, dealing with the laws of nature, the other practical, dealing with concrete individual sick patients. As we study the 17th century medicine we will see how widely divergent were these two worlds. (We do not ordinarily appreciate how divergent they still are.)

Riverius considered that all parts of medicine can be derived from the axioms and principles. To be sure, some parts might be conjectural and the principles not altogether certain, but this does not mean that medicine is an art rather than a science, any more than physics should be considered an art merely because some of its proofs are conjectural and not drawn from true causes perfectly shown. If details were still to be worked out, the incomplete knowledge did not impair the validity of the principles.

Medicine, then, must be viewed under two different aspects: pure knowledge, a branch of philosophy comparable to physics; and the practical activity concerned with maintaining or restoring health. For the rationalist, reason is the tool by which we achieve truth. The universe is rational throughout. Reason can perceive the interrelationships, and discover the basic principles which underlie the universe. In identifying the first principles we necessarily start, at least to some degree, with empirical observations. There is the basic supposition that any particular event, any concrete datum, manifests universal principles; furthermore, that reason can detect the universals which lurk in the particular. Reason can analyse the particular, find the universals, discern their logical connections, and thus arrive at scientific truth. The philosopher can rise from the particular to the universal.

Knowledge, the subject matter of science, requires demonstration. For the Galenists this was largely a logical process. The scientist sought first principles that can serve as a source from which concrete particulars can be logically derived. The philosopher tried to identify these first principles by rational analysis. He usually came up with assertions which logicians call 'analytic', that is, making explicit what was already implicit, but without adding any new information not already present. As an example of this analytical procedure, we can use the following two propositions. Let us say that a man looks at his watch and says, 'It is 9:37.' His companion says, 'Yes, it is 23 minutes before 10.' These are equivalent statements; one is derived from the other by analysis. Much of 17th century medical science and its principles were comparable.

Another feature of 17th century Galenic medicine was its systematic character. True or valid science must be capable of explaining all concrete phenomena through basic propositions and fundamental underlying concepts. These concepts and their derivatives formed a unified whole. Areas of ignorance were merely lacunae whose existence did not affect the validity of the whole. Lacunae did indeed indicate imperfection of knowledge, but this need not cause any surprise in this sublunary sphere where contingency plays such an important role. Science was a unified whole with interconnection of parts and was not overthrown by particular uncertainties or gaps in knowledge. The systematic and unified character of science represented an article of faith with Riverius and his fellow Galenists.

II

The teachings of Riverius follow quite closely the doctrines of Galen, but it is desirable to have a 17th century re-statement for comparison with other competing doctrines. Riverius built his system on the four ultimate elements—earth, air, fire, and water—and the four primary qualities—the hot, the cold, the moist, and the dry.[9] The qualities applied to the elements in pairs: air was warm and moist; water, cold and moist; earth, cold and dry; fire, hot and dry. There were also secondary qualities, such as rare, dense, heavy, hard, coarse, etc., which arose from combinations of primary qualities.

Human physiology rested on the humours and the temperaments.[10] The primary humours are the familiar blood, phlegm, yellow bile, and black bile. There are also secondary humours, which we will discuss later. The temperaments depend on the *proportion* of the primary qualities. Distinguishing one temperament from another was the predominance of one or another of the qualities. Thus, he distinguished four 'simple' temperaments, in which pairs of qualities predominate—the warm and moist, the warm and dry, the cold and moist, and the cold and dry. And finally, in one temperament all the qualities were duly proportioned, with none of them dominant, neither singly nor in combination.

We will repeatedly meet the concept of 'due proportion'. All bodies must have a certain proportion and interrelationship of the

elements and qualities. One or another may predominate, or there may be certain combinations which prevail or a completely neutral mixture with none predominating. A *normal* range or proportionality was recognized. Dominance must stay within limits in order to be consistent with health. If some quality gets beyond the 'due' proportion, the normal becomes pathological. But what constitutes the due proportion is not made explicit. There is no numerical expression. Indeed, the need for quantitative determination was not felt.

The elements and the primary qualities are metaphysical, not demonstrable to the senses. However, the humours, another basic concept, were material and at least partially demonstrable. No sharp line divided what was clearly perceptible, e.g., the secretion of phlegm, from an abstraction such as the Hot or the Cold. The humours and qualities were all deemed equally real and equally available for the conceptual framework.

The primary humours—the blood, phlegm, the yellow bile and the black bile—derived from the food. They received their Form (i.e., their characteristic properties) in the liver, were contained in the veins and through the veins were carried to the solid parts.[11]

Secondary humours were produced as the bodily parts acted on the primary fluids. These secondary humours, also four in number, represented the successive changes that took place when the primary humours were assimilated into the tissues. Riverius distinguished them as the 'innominate', dew, gluten, and cambium, and identified them partly by observation, partly by inference.[12] Thus, the dew and the gluten represent what we would call tissue juices, quite distinct from the primary humours. The cambium can be inferred from analogy with plants and also from the data of wound healing and repair.

Since the body tissues derive from the blood, obviously some change must take place whereby the blood became transformed into the tissue substance. Riverius arbitrarily divided this change into four stages: a beginning, which took place in the smallest or ultimate veins; an elaboration which he called dew; a further modification as the changed substance began to unite with the solid parts, a stage which he called gluten; and finally the cambium, as the material was converted and assimilated to the part.

Another basic concept of Riverius was *excretion*. What is not useful for nutrition must be separated off and excluded. In the first digestion the nutrient material becomes the humours while the faeces represent

the excrement. The primary humours are produced in the liver and contained in the blood. When the primary humours are utilized for nutrition of the parts the second digestion occurs, and any residue gets excreted.[13]

The excremental humours are of different types. There is the yellow bile, excreted into the gall bladder. While not suitable for nutrition, it has the function of stimulating the intestine to contract and of eliminating from the intestines the adherent mucus. There is also black bile, which cannot be converted into nutritious material. It is present in the spleen, and is transferred to the stomach through the *vasa breva*. Hot and sharp, it helps digestion in the stomach.

The 'serum' is the excrement eliminated in the urine and sweat. It represents the excess fluid in food, excreted after the liver had done its work. It serves as a vehicle for readier transportation of substances through the body.

Then, there is the excrementitious phlegm—what cannot be digested but is excreted. It collects in the brain and includes many vapours which arise upward and are condensed by the coldness of the brain. The phlegm got discharged through the mouth and nose. The collected phlegm may be insipid or sweet, acid or salt; for example, an acid phlegm is made by a more intense cold, while the salt phlegm arises from corruption (*putredine*). Phlegm has various degrees of thinness and thickness. Riverius distinguished four categories, thin, thick, glassy, and stony.[14]

The concept of *spirit* formed an important part of the Galenic tradition and of all Western philosophy and medicine. According to Hippocrates the body was composed of the solid parts, the humours, and the spirits. These latter were responsible for movement, and thus represented an activating factor. A major problem, both scientific and philosophical, has always concerned the source of movement. One significant answer was, that spirits constituted the active element and provided motion for inert substance.

There were some compelling reasons for holding the concept of spirits. One is that body and soul are entirely separate, and consequently cannot have any direct connection with each other. For soul to act on body there must be an intermediate substance, sharing in the properties of both. Spirit formed such an intermediary. Then,

too, where soul was not involved, spirit represented the vivifying element present, e.g., in a prolific seed (*semen prolificum*) and absent from the inert seed. And furthermore, to make confusion worse, we must, to exist, have an aereal substance—'*spiritus sive aëris attractio*'.[15]

Quite obviously the single word 'spirit' embraced a variety of meanings in a quite confused whole. The confusion rests principally in the fact that 'spirit' has two different major senses. One concerns the completely immaterial, as in the concept that God is spirit, or the soul is spirit, or the mind is of a spiritual nature. These all imply a sharp distinction from something concrete and gross which we call material. On the other hand, 'spirit' also conveys the sense of extremely fine matter, the finest possible materiality, far more delicate than any crude lumpy substance which 'matter' ordinarily implies. The value of the term 'spirit' rests with this very ambiguity. It shares in properties of 'pure' spirit and also of fine matter. The use of the word permits us to go readily from the immaterial substance to the material substance, and thus gives us a sense of achievement.

The Galenic tradition distinguished an innate spirit, *insitus*, pervading all parts. Poorly defined, it was the approximate equivalent of life in the most general sense. It went along with the innate heat and the primitive moisture. These concepts of innate heat, primitive moisture, and innate spirit distinguished the basic properties of life. In addition to the innate spirit, however, there are the specific spirits, familiar from the Galenic teachings—the natural, the vital, and the animal. These spirits are definitely material in character, relatively localized, and are produced in specific organs, namely, the liver, the heart, and the brain, respectively. We must attend particularly to the latter meaning of spirit—finely material and an important part of the bodily physiology.

Physicians found it much easier to observe the fluid parts of the body than the solid parts. Nevertheless, certain important distinctions among the solids made up a foundation for understanding disease. The solid parts—the 'parts'—were of two sorts (i.e., they fell into two genera).[16] The *similar* parts were those which have an essentially homogeneous nature and appearance. Thus, all parts of nerve were nerve and they all appeared similar one to the other, or sufficiently so that the differences were not important. And so, too, with bone, and 'flesh'

(muscle), and membranes. There was, thus, a rough correspondence between similar parts and what we would call 'tissues'. The pale tissues derived from the masculine seed, the red tissues from the feminine blood.

If similar parts are to function they must have as a necessary condition a suitable nutrition (or what we would call metabolism). That is, they must attract appropriate nourishment, retain and assimilate it and excrete what is not usable. In this sense, all similar parts had the 'function' of nutrition.

The 'dissimilar' parts were the organs which have a composite structure of many 'similar' parts. Here we see a crude distinction of tissues and organs in the modern sense. Now organs, to function properly, require considerably more than merely proper metabolism. They must satisfy certain requirements—they must have a suitable size, the proper number of components, and appropriate connections. Organs must have a suitable configuration or shape, they must have suitable cavities or lumens where appropriate, and they must possess a suitable degree of roughness or smoothness.

For each organ there is a suitable norm. To function properly the organ must have an 'appropriate' or suitable size, shape, connection, configuration, etc. These constituted the *properties* of healthy organs. As we shall see, disease occurred when these properties deviated from the healthy state, to a degree that impaired their function. But how much may they deviate? What *is* the normal? Riverius did not provide any specific quantitative measure. The only quantification offered was the rough evaluation of 'due' or 'appropriate' or 'proper'. Health was the *proper* mingling of elements in due proportion, with the *proper* performance of function, and possession of *proper* attributes. Disease occurred when the qualities, properties, attributes, diverged excessively from the normal. The physician—the scientific physician—tried to analyse the ways in which deviation occurred. The analysis took the form of identifying properties which might be either normal or abnormal, such as size, shape, smoothness.

In the Aristotelian tradition, all objects consist of matter and form. The Form of man is the soul and the soul is the form of the body. In Aristotelian philosophy all dynamic activity depends on the Form; the soul is thus the cause and first principle of all actions of living bodies. The *faculties* represent the agencies, inseparable from the soul,

through which the soul brings about particular functions in the body. In other words, the faculty is the property by which the soul, or anima (or Form) exercises its function in the body. However, Form and soul are immaterial. Hence, we again have the 'third man' concept—between the immaterial form and the material components there exist the faculties or intermediate entities. The faculties are specific activities mediated by the spirits which are finely material.

Riverius indicated three major categories of spirit, and since each of them performs different functions, each has its own faculties. The natural faculties, functioning through the natural spirits, are, as Riverius said,[17] that power of the soul by which the body is nourished, grows, and reproduces. The three major subdivisions of the natural faculties are thus nutrition, growth, and generation. Riverius took up each of these subdivisions in turn, but for our purposes it is unnecessary to follow him.

One distinction, however, has extreme importance. Under the nutritive faculty—the basic metabolic activity—he distinguised between 'private' and 'public' functioning. The private functioning involves only the part itself—the similar parts are concerned with their own nutrition only—but the dissimilar or organic parts may be concerned with the nutrition of the whole body. Thus, an individual part, such as a nerve or a bone, need only nourish itself, without responsibility for the rest of the body, but the stomach or intestines or kidney have a 'public' function in that they must function properly for the sake of the entire body.

In a vague fashion Riverius thus distinguished two kinds of function, one of which, as already mentioned, we might properly call metabolism if we use present-day terminology. The other concerned the more readily observed organ activity. The stomach and intestines, e.g., receiving the swallowed food, transformed it into chyle, and the liver, in turn, transformed this into blood. The lungs took in air, the kidneys excreted urine, and so on. These demonstrable activities could be correlated with organs, and even with parts of organs. The factors responsible for this could, in part, be observed grossly.

So far as private function is concerned, Riverius could not correlate the 'intimate' metabolic activity with any empirically observed features. The attraction, retention, digestion, and expulsion took place within the tissues themselves (or, as we might say today, within the cells). Consequently, he attributed the private functions to metaphysical

agencies, namely, the primary qualities, particularly the hot and the dry.[18]

On the other hand, organ activity lent itself much better to concrete correlation. Thus, in the intestinal tract Riverius was familiar with the different muscular coats and the three 'kinds' of muscle fibres, the circular, the straight, and the oblique. The public functions, he thought, were accomplished in part by these fibres: the straight fibres take a part in 'attraction', while the 'retention' faculty involved the oblique fibres, and excretion he associated with the transverse fibres. Riverius was thus making a correlation between empirically observed functions and empirically observed physical structure. *We see here the thorough intermingling of what I would call the physical and metaphysical factors in explaining physiological activity.*

To render his observations orderly, Riverius used various concepts and made various distinctions. He had, however, sufficient critical awareness to realize that his explanations were not always satisfactory and his categories not always adequate. Thus, he indicated that certain activities of faculties are accomplished by the hot and the dry.

> 'It is not to be wondered at that attraction, retention, and expulsion, actions so very diverse, should be accompanied by the same qualities, namely, heat and dryness. For this demonstrates that we must recognize a higher cause of action than proportion (*temperies*), which is only the instrument of the Form. Thus, by one and the same instrument an artist may achieve quite different works, and it is not to be denied that different grades of heat or dryness may be required for this or that action. But these are as yet unknown to us.'[19]

Riverius was saying that here we have many different functions, apparently quite diverse, and that he was attributing them all to the same two properties of heat and dryness. The 'temperate' or proper proportion gave a verbal explanation for the activity. Riverius realized that this was inadequate and that some 'higher' cause must be found. But he did not have any concept of what this might be. In other words, he needed some new categories, but he did not know where to find them. Such new categories could only come from new observations, new empirical investigations. And he had no new facts, no new data, and no new concepts.

Riverius devoted surprisingly little space to the vital faculties.[20] His exposition of the heart, lungs, and blood vessels followed the Galenic teachings. The vital spirits were formed in the heart and flowed into

the arteries. Ministering to the vital faculty were the pulse and the respiration. The pulse, with its systole and diastole, arose from the pulsific faculty of the heart and distributed the spirits over the entire body. The heart and arteries were filled in diastole. As the heart dilated it drew air from the lungs through the 'venous artery' [pulmonary veins] and also blood from the vena cava. These were mixed in the left side of the heart and the spirits were generated.

The dilation of the arteries drew the spirits from the heart [an attraction] and also drew in air from the outside through minute openings which end in the skin. This external air tempered the internal heat. In systole, or contraction, the heart excreted the smoky excrement that arose in the production of the spirits, and expelled it through the venous artery into the lungs. The arteries also expelled smoky vapours through the insensible perspiration.

The two auricles in the heart dilate as the heart is constricted, the right containing the blood, the left, spirits. Riverius made no mention of the way the blood might get from the right heart to the left. He did not mention any pores through the septum nor did he mention any transit through the lungs themselves.

The pulse required a pulsific faculty as the prime agent; it also required suitable 'instruments' (the heart and arteries); and finally, a usefulness or need (i.e., a purpose or final cause) which incited the faculty to action. This final cause was the production of the spirits and the preservation of the native heat. The lungs took part in this process of providing the vital spirits, drawing in the air during inspiration and expelling the warm and smoky vapours in expiration.

Riverius devoted considerably more space to the animal faculties[21]—which concerned the mind and its activities. He distinguished three functions of the mind. The first he called 'imagination', which included what we would call perception as distinguished from reasoning, judging or discrimination. Imagination combined the separate sensations of different sense modalities and also had our present-day meaning of 'phantasy'. Riverius discussed briefly the 'common sensory' where the integration of sensations, together with past images, took place.

A second function of mind, unique to man, was reasoning, through which man discourses, understands, and reasons. And the third mental function was memory.

As an example of neurophysiology we can indicate Riverius' discusion of sleep.[22] He distinguished the external senses—the sense

organs—from the internal senses. By these he meant the reception of images and the integration in the imagination. The activity whereby images and reception were carried to the imagination ceased during sleep, although the imagination itself, as well as reasoning and remembering, might still function.

The immediate cause of sleep was anything that would prevent the animal spirits from flowing. This cessation would take place if vapours blocked the brain and obstructed the channels. Normally, from the food, a dew-like vapour was carried upward into the brain and flowed through the ventricles and channels, to hinder the spirits and lull the activities. Excessive eating and drinking would thus produce a longer and more profound sleep. Those who were very sober or underwent fasts slept but little.

Many things could prevent the inflow of spirits, dissipate or check them in some way. Thus, long physical activity, or intensive thought and study, could dissipate the spirits so that the normal supply did not suffice. Or cold could check the flow of spirits, and so also could gentle sounds or monotony, such as the flow of water or quiet music. Sleep came to an end when the flow of animal spirits was restored. Sleep, an interrupted flow of animal spirits, might be produced by some blockage of the channels or by insufficient quantity of spirits, variously induced.

III

We can gain considerable insight into some 17th century thought modes if we examine carefully a few specific examples of the way Riverius explained phenomena. Riverius combined observations with reasoning, but he did so within the over-arching framework which determined what he should observe and the way in which he should handle his observations.

Take, for example, his discussion of the relation between temperament and sex.[23] In general, he said men are warm and dry, while women are cold and moist. What is the evidence for such a generalization? Today we would try to provide direct empirical evidence. Riverius, however, justified the statement by reason. 'The truth of this theorem is easily demonstrated,' he declared, and he presented a

chain of 'evidence' that rested on authority, conjecture and inference, with only a minimum direct empirical basis.

He said that the seed from which males are generated is warmer than that which produces the females. Hippocrates had declared that males arise on the right, females on the left. Anatomy indicates that the vessel on the right which supplies the seminal material arises directly from the trunk of the vena cava, and *therefore* [italics mine] the seed on the right is warmer; the left vessel [spermatic vein] arises not from the vena cava itself but from the 'emulgent branch' [renal vein] and *therefore* the semen on that side is colder. Furthermore, the males are much stronger and capable of working harder, and *therefore* they are warmer. The fact that males have a stronger pulse and respiration, a deeper and more intense voice, 'indicates' (*arguere*) a greater heat. Moreover, the excretions are less in males than in females, since in women there are the menstrual 'purgations'. This *indicates* that in the female the heat is not sufficient to dissipate the entire mass of blood that is produced. In males, however, all the blood is disposed of and no superfluity remains to be excreted, as occurs in women. Menstruation, therefore, was adduced as evidence of diminished quantity of heat in the female relative to the male. However, Riverius indicated that, although in general males are warmer, there are exceptions, and some females are warmer than some males.

Riverius has provided certain empirical correlations between sex and bodily features. His fault lay in attempting to connect up his observed data and his hypothetical concepts. He tried to explain the observed data by referring them to abstractions and to concepts that, to our minds, are entirely irrelevant, i.e., they have no inferential connection, and indicate confusion of terms. However, he obviously thought that relevance existed, as well as logical clarity.

A quite different example of reasoning we find in his concern with the question, why are not all men of equal height?[24] The observation that men do differ in height is unassailable. But why does this phenomenon occur? Riverius proffered three reasons, or explanations. The first he derived from the more basic principle that bodies differ in their composition and characteristics. Bodies that are more moist and more warm were more suitable for growth and grew more rapidly than did the dry and cold. As a second principle he invoked nutrition. Bodies more abundantly nourished grow more rapidly than those with imperfect and more scanty nutrition. And the third explanation derived

from the similarity to the parents. He relied on the observation that children tend to resemble their parents, and he related this phenomenon to the seed and its role in generation.

His explanations, drawn from empirical observations, apply to many biological subjects. They are not *ad hoc* concepts, appropriate only to the immediate problem. Instead, what he said about men could equally apply to plants or animals. He had identified certain general conditions of growth, involving environmental and genetic factors, and indicated their relevance to the problem at hand.

Discussing the influence of heavenly bodies, Riverius made some significant exposition in regard to the moon.[25] Talking about the problems of pathology and the so-called 'critical days' when the crisis of disease would occur, he indicated in empirical fashion the importance of the seventh, fourteenth, and twenty-first day. All this rested in part on authority, in part on observation. The causes of the crisis were multiple. During the crisis the expulsive faculty drove out the irritating material, and the question arose why this took place on the seventh and fourteenth day more than at other times. He considered the responsible factor to be the cycle of the moon. The moon, he made perfectly clear, had 'dominion' in this world, especially over moist bodies. The influence, however, was not the simple effect of the quarter of the moon manifest at a given time, that is, the relationship between the moon and the sun. Were this the only factor the moon would have equal influence over all people. Actually, the influence was far more complicated, and was related not to the sun alone but to the signs of the zodiac. His reasoning is clear. If crises were dependent on the phases of the moon relative to the sun, then all patients who got sick on a certain day should have their crisis at the same time. This was obviously not the case. Therefore, we must connect up the moon in a more subtle and complicated manner and this led to the place in the zodiac in which the illness first began.

I offer this as an interesting example of scientific method, whereby he combined observation and reasoning, and indicated a basic critical acumen. We start with the principle that the moon has an effect on human affairs. This belief, in turn, rested upon evidence, such as the periodicity of some human activities in approximately twenty-eight days, e.g., the menstrual cycle. Assuming that the moon had an effect, further observation showed that the effect was not a simple one. Since a simple correlation was not adequate, Riverius searched for further

factors to provide a more adequate or satisfying correlation. This demanded a greater complexity, which he found in the zodiac. From our standpoint the fatal drawback was the failure to demonstrate that a correlation existed in fact between the evidence of the zodiac and the observed frequency of crisis. But within the rigid conceptual system that enveloped him, he was showing definite critical acumen.

Riverius discussed dreams, wherein he distinguished the natural from the supernatural.[26] The latter, which are either of divine or diabolical origin, he relegated to metaphysics and theology. He insisted that he was concerned only with natural phenomena. This brief interjection, I believe, has considerable significance in explaining the general point of view of the mid-17th century. The supernatural world was very real, and the scientists had to make a specific disclaimer, that they were not considering this aspect of reality. Riverius emphasized that he was concerned only with natural dreams and these he connected up with natural phenomena. He felt that the kind of dreams that people have is related to their temperament. Then, characteristically, he added a brief note that the stars may concur with the temperament and bodily disposition, to bring about dreams, and this astral influence might have significance.

IV

Disease, the subject matter of pathology, is not easy to define. It is the opposite of health, and health involves particularly the proper functioning of the body. Disease, then, necessarily involves some manifest impairment of function, and this impairment must have a significant duration. Furthermore, an impaired function must be evaluated in relation to the normal for that person, and this depends on his natural constitution. Riverius thus considered disease an abnormal disposition which impaired the bodily function. All disease must have a degree of constancy in the body to distinguish it from a simple affection or ailment. Furthermore, impaired function must immediately and directly and intrinsically follow any disease.[27]

Disease had certain properties, which, in the scholastic terminology, concerned either the essence of the disease or else the 'accidents' (non-essential factors). The essential characteristics are those which make an

entity what it is. Among diseases there are three major categories of 'genera', which bore the somewhat barbarous names of 'similar', 'organic', and 'diseases of union' (*communis*).[28] These divisions followed directly from the physiological concepts which Riverius had previously discussed.

For Riverius pathology had to establish a framework whereby the logical position of a disease could be identified. Today, when we examine a patient, we try to make a preliminary classification whether the condition is, for example, infectious or metabolic, neoplastic or inflammatory. Riverius had the same attitude, but he had different categories. He wanted to know whether the disease was one which affected the similar parts or the organs, whether it affected the humours primarily or whether it had some other fundamental character.

'Similar disease' affected the similar parts, as already described. These, we recall, represented the uniform components of the body, analogous to our present-day tissues. These all had the common 'function' of nutrition. Hence, since disease impairs function, 'similar disease' in some manner will impair the nutritive function. Nutrition normally took place through the proper temper or proportion of the primary qualities—the hot, the cold, the moist, and the dry. Hence, for similar parts any disproportion (*intemperies*) or pathological relationship of the qualities constituted disease. The major subdivisions of 'similar diseases' revolved around the four qualities, separate or in combination. One disease would be a disproportion in regard to heat, another in regard to the cold and moist and so on.[29]

In addition to the essential characteristics which identified the disease, other properties helped to define the particular disease but did not involve the fundamental nature by which it was identified. Given a particular disproportion, the cause might vary—in one case it might be an inflammation, or in another a fever. Those causal factors which we today regard as so important, Riverius regarded as 'accidental' (in the sense of formal logic). What, for example, actually 'caused' the inflammation he did not consider particularly significant.

The second major category Riverius called 'organic',[30] that is, concerned with organs, in distinction to our present-day tissues. Again he was following the physiological analysis given previously. Diseases of organs depended on impaired function and an organ, we recall, had 'public' function pertaining to the entire body.

To understand the function of an organ we must understand its

properties. Any organ, for its proper functioning, must have a suitable configuration, size, number of components and connections. Consequently, impairment of any of these categories could induce disease. To analyse diseases we needed to identify the particular aspect that was impaired. Organic diseases had four basic genera, corresponding to their properties: diseases of configuration, of size, of number, and of connection. Each of these had subdivisions. He had to identify the genus and the subdivisions. Thus, in the genus diseases of configuration, the abnormality might involve the shape; the smoothness (e.g., a part naturally rugose becoming smooth, or vice versa); or the defect would involve the channels or cavities it contained (as, a dilatation or obstruction of a normally present lumen). Diseases of size occurred when an organ was either too large or too small. For disease involving a number of parts the presence of six fingers on a hand would be an example, or the absence of one kidney. A disease of connection meant that parts which should have been separate were joined together, or parts which should have been joined together were separate, as, e.g., a dislocation or a herniation.

The third genus is that of continuity, where an ulcer or an incision or a fracture or an evulsion take place. He subdivided these diseases according to the active agent, whether it was produced by a sharp cutting instrument, an eroding or acrid caustic substance, a *h*eavy blunt agent, or a tearing evulsing process. He indicated, incidentally, that an evulsion, as of a finger, occurred most often in torture—an interesting sidelight on the cultural environment of medicine.[31] The various diseases in these subdivisions would be called incisions, wounds, ulcer, fracture, puncture, rupture, etc.

I must emphasize that for Riverius the important thing was to identify the *kind* of disease he was dealing with. We have the same problem today, but we have a different classification of kinds—a different nosology—than he did. His first problem was to identify the major (generic) kinds and establish definitions. This he did by indicating the essence or essential features of the disease, the factors which constituted the disease in question. Diseases, however, have other properties which help us classify them more accurately and yet do not alter the essential nature. These terms or predicates, which the scholastics called *accidents*, are relevant categories which may be meaningfully applied to any disease but would not alter its nature.

Once we have the essence of the disease, we can characterize it

further and thus help our understanding. For example, a disease may be severe or slight; it may be identified in one or another different phase, whether at incipience, or in a stage of growth, or one of decline. It may last a long time or a short time. It may be called acute, subacute, or chronic; it may be continuous or intermittent, benign or malignant. It may be without danger, or very threatening or even lethal. All of these properties can be meaningfully applied to a disease, *but they do not affect the essence*. The particular way in which these were arranged in a logical series did not alter the principles involved. The scholastic nomenclature, of genus and species, attempted to preserve a hierarchal relationship and to give the terms a metaphysical status.

Quite significant are his discussions of cause. He included cause among the accidents, rather than the essence. That is to say, for him *any particular causal factor did not affect the basic nature of the disease nor change its classification*. What we call 'causes' simply provided additional meaning or information, which helped us understand the phenomenon but did not change the intrinsic nature.

His definition of cause was extraordinarily broad, comprising nothing less than all factors which have to do with the production of disease.[32] Everything must be considered a cause which concerns the production of the disease in any manner, whether essentially or accidentally, mediately or immediately, as well as anything which had to do with maintaining or increasing the disease. These were all to be considered the 'efficient cause' in the sense of Aristotle.

In his analysis he provided various differential features to distinguish[33] one genus of cause from another. These all represented various properties of events or things, and indicated different ways of using the *word* 'cause' in meaningful fashion. Or, to express the matter differently, he provided what is essentially a verbal or grammatical analysis which showed how the word 'cause' can be utilized in different fashions, and he tried to indicate some of these differences. He was not concerned with what we would call empirical analysis of cause, nor with establishing correlations which would hold up in a statistical analysis. He was concerned merely with logical, grammatical, or rational connexions, connexions which were set out *a priori*, by armchair philosophizing.

The word 'cause' can be applied to events in various ways. Riverius considered the following, among others: *essential*, that which acts by its own indwelling force, without the intervention of anything else; this he contrasted with *accidental*, which acts through the intervention

of something else. Or, the term could be regarded in the sense of a *principal* factor or an *accessory* factor or as an *indispensable* factor in a particular event. He gave the example of arthritis. The principal cause of the effusion into the joints was the cold constitution of the air and abundant excremental humour; the accessory cause was the delicacy of the humours; and the indispensable cause, a weakness of the joints and looseness of the cavities. Such an analysis, clearly, represented an armchair and verbal analysis rather than an empirical investigation.

Causes might be considered as remote or proximal, external or internal, which are self-explanatory. Under external causes he made a further distinction into the necessary and the non-necessary. The necessary causes were those which we cannot avoid but nevertheless need not bring disease in their train. When appropriate, they could preserve health, but by their abuse they might induce disease. Under this category he listed the so-called 'non-naturals', the environmental factors applicable to either health or disease. He enumerated the traditional six non-naturals: air; food and drink; movement and rest; the excreta and retenta; sleep and wakefulness; and affections of the mind. It required a little juggling to count all of these as 'external' causes, but he could readily place things in a logical straitjacket.

The doctrine of the non-naturals had significance in stressing the role that environment might play in various diseases. Riverius paid little attention to what we think of as specific factors, nor did he, in the present work, try to relate specific factors to specific diseases. He preferred to establish a formal logical framework in which different aspects would have a suitable place, rather than to establish actual correlations.

Other species of cause included such terms as antecedent cause or the conjoint cause or the continuing (containing) cause. For example, if there were an obstruction due to a calculus, the disease, he thought, was removed when the calculus (the continuing cause) was removed.

Distinctions of this type do not help us understand individual diseases but they do illustrate the various ways in which the *word* 'cause' was used. They also make us appreciate the confusion attending that concept. Establishing a verbal hierarchic arrangement did not render the concepts clear.

Riverius provided 'causal' analyses for the three major categories of disease. One chapter he devoted to the causes of similar diseases,[34] which, we recall, represented conditions produced by disproportion in

the primary qualities. He became concrete in listing various factors which might cause disproportion, or *intemperance*, of each quality in turn. An intemperance in regard to heat could be produced by movement, by corruption, by proximity or contact with a warm body, by constipation, or by hot foods producing hot humours. In all this he merely enumerated possibilities, with no logical force. We must note the lack of discrimination, and the mingling of such factors as physical heat (as from a fire) with 'hot' foods such as garlic or onions which produce 'hot' humours.

Riverius gave comparable analyses for other genera of similar diseases. For disproportion of the cold quality, he indicated as causal factors the proximity to a cold body, or the quality or quantity of the food ingested, lack of exercise, etc. And he provided other reasons for disproportions of the dry and of the moist. We are not concerned with the specific analysis but rather with the mode of thought that he displayed.

When he took up organic diseases and their causes,[35] he tried to provide categories which could account for the specific changes in configuration, shape, position, connexion, etc. Thus, he implicated a weakness of the generating or formative power, or a hereditary disposition, or a failure of quantity or quality. He indicated that causes could be external or internal, could be due to trauma or alterations in the humours. Indeed, in discussing one category involving obstruction, constriction or dilatation, he declared that it would be impossible to enumerate all the causal factors individually.

Riverius mentioned some of the ways in which the particular features *might* be produced, but he was not concerned with finding the specific factors that actually, in point of fact, *did* bring about a particular condition. He merely recognized that there were innumerable contributory factors.

The really important task, for him, was proper categorization. A given patient had to be assigned to the correct category and identified as exemplifying, say, a disease of obstruction. In any particular case the concrete factor that brought about the obstruction was a matter of practical importance, not of science. It would not allow for generalization nor, he thought, for arrangement in logical categories. Science dealt with universals, while practical art dealt with particulars. The *disease* lay in the obstruction. That the obstruction might be due in one case to a stone, in another case to a stricture, was an 'accidental'

and not an 'essential' feature. There was no realization that continued observation could provide inductive generalization, nor did he realize that by observing enough patients with obstruction he *could* elicit general rules.

The basic attitude was that the truth could be discovered by rational analysis. Francis Bacon, of course, had pleaded for inductive generalization, for the collection of instances and the attempt to find the rules implicit therein. Riverius offered the type of science against which Francis Bacon was disputing. The rationalist would start with experience, but he did not maintain that systematic experimental investigation was the proper means of utilizing experience.

We have a conflict of values. For Riverius the important value was maintaining a logical framework and careful definition, with series of differentiae based on logical discrimination. The empirical and experimental attitude, on the other hand, held that the important feature was the accumulation of observations, and that logical categorization was less important.

Riverius, when he tried to analyse causal factors, engaged in an armchair analysis. He hunted for further discriminations, all on the basis of data with which he was familiar. He did not realize that new empirical data could require a new conceptual framework; and, similarly, that improving an inadequate framework could require new conceptual tools. He was in the position of a parent who clothed a small child in a garment, and then tried to have the child wear the same garment despite vigorous growth. The old garment might cover for a while, and could even allow some stretching, but nevertheless, with continued growth, new material would be necessary to provide a decent covering.

Riverius had a detailed and complex systematization of all medical phenomena, but it is not my purpose to epitomize his entire system. To understand the medical developments of the 18th century we must keep in mind two different aspects—first, the factual content of the 17th century texts, and then the thought modes that characterized the individual authors. In this chapter I have tried to present some salient features of Riverius—his essentially Galenic content and his particular brand of rationalistic method. But while our present-day scientific temper tends to condemn—or at least scorn—17th century

rationalism, we should remember that actually men like Riverius were also keen observers hampered by a sort of intellectual straitjacket.

Order and system and logic were prime values, that laid special emphasis on categorization. Categorization is not a mere exercise in abstraction or logic chopping. It involved an intense interest in description and arrangement. There was a thorough passion for order, with the faith that the universe was orderly; that an insight into the real order would enable us to arrange all phenomena, with nothing left out; that a proper arrangement would harmonize with the total hierarchy of Being.

With such an implicit background we need not wonder that the scheme of values then prevalent differed quite substantially from our own. Riverius was concerned with knowing what manner of disease he was facing, what was its place in the total scheme of Being. He wanted to know genus and differentiae, essence and accident, and the relation to Aristotelian categories. His enormous faith in the Aristotelian analysis reflected his strongly conservative bent, which hindered his appreciation or acceptance of new doctrines.

Indeed, the entire last half of the 17th century might be construed as the battle between the ancients and the moderns. Riverius represented the ancients, and he ably carried their banner. But the very rigidity of his approach precluded any adaptation to new circumstances. And new circumstances crowded in on the medical world with great vigour and inexorable insistence. Riverius, so to speak, represented the base line on which 17th century progress developed. Let us study further the new doctrines and the new modes of intellectual adaptation.

VAN HELMONT

Riverius was a conformist, an adherent to the traditions of classical medicine. Van Helmont, on the other hand, was a rebel. He explicitly condemned the philosophy of Aristotle and the scholastics as well as the traditional medicine of the 'Schools'. He claimed to propound new doctrines. In part he actually did so, but in part he merely covered over old concepts with new garments. He was a profound thinker, an excellent experimentalist, and for his time an amazingly good chemist.

But he also represents the picture of a man groping in the dark, hemmed in by conceptual bonds which he was powerless to break.

Jean-Baptiste van Helmont was born in Brussels in 1577, of a well-to-do upper-class family. Precocious and talented, he first studied philosophy in Louvain, but then he turned to medicine, despite family opposition. Reading prodigiously, he completed his medical studies in 1599, and then taught for a while. However, dissatisfied with prevalent medical doctrines, he sought something more satisfying. He travelled extensively for a period of about ten years, during which time he devoted himself more and more to chemistry. After marrying a rich wife in 1609, he retired to Vilvorde, near Brussels, where he pursued chemical experiments, engaged in medical practice, and wrote many treatises. He died in 1644. His major works and collected writings were published only after his death, as the *Ortus Medicinae* (Amsterdam, 1648), re-published in various editions during the 17th century.

If we had to characterize in a few words the basic components of Helmontian thought, I would first think of the religious aspects, which Pagel[1] has especially emphasized. Then, I would stress his regard for the dynamic 'spiritual' aspects of reality—*process*, *events*, *activity*, and the forces underlying these. He was concerned with aspects of mind and the importance of Ideas, taken in their Platonic sense. And after that, I would think of him as a natural scientist, devoting himself especially to living creatures and the organic aspects of the world, who tried to connect up chemical concepts with both physiology and the doctrines of philosophy.

The dominant philosophy—the philosophy of the 'Schools'—was Aristotelianism, and the dominant medical theory was Galenism. Some aspects of the 16th century Galenism we have discussed in the last chapter. Like Paracelsus before him, van Helmont was quite dissatisfied with the conceptual tools available to explain phenomena. As a chemist van Helmont had studied many phenomena unknown to Galenic physicians or, if known, quite ignored. He had a wide range of experiences, many of them quite new, all crying out for explanation. The prevalent concepts—the elements and the primary qualities, the humours and temperaments, the properties and faculties, matter and form—all seemed unsatisfactory to explain natural phenomena. But if he wanted to find alternative explanations he had to create them anew, or draw them from predecessors.

Van Helmont owed a heavy debt to Paracelsus, and Pagel, particu-

larly, has discussed this aspect.[2] I wish also to emphasize the general neoplatonic background of Paracelsus' thought,[3] and indicate that van Helmont had similar neoplatonic leanings, as we shall see.

With his prolix and complicated Latin, van Helmont[4] is extraordinarily difficult to read. His style is highly metaphorical, he invented new terms, often with mystical overtones. In his expositions he was not in any way systematic. While the separate treatises in his book purport to deal each with a different subject, they do so only in the most approximate fashion. The expositions ramble. There is some degree of progression from general philosophic principles to physiology to pathology, but the path is most irregular and rugged.

The only English translation of his writings, published in 1662,[5] is an utterly fantastic piece of composition. It has all the faults of the worst 17th century English style, a disregard of syntax and of niceties in medical terminology. Immediately after its publication Marchmont Nedham,[6] while praising its content, declared it very hard to read; indeed, reading it 'ten times over is too little'. And this difficulty has not diminished with time.

To read van Helmont exclusively in Latin requires considerable virtuosity, and yet the English translation, by itself, cannot provide any reliable insight into van Helmont. Most readers will want to use the English translation only as a 'finder' and then must consult the Latin to achieve an understanding.

II

Van Helmont provided a basic metaphysical analysis, on which rested his medical doctrines, both physiological and pathological. He specifically rejected the familiar basic concepts which played such an important part in Galenism. For example, he expressly denied the existence of Aristotle's indeterminate Matter [*hyle*] and Form. He denied the long accepted teachings of the four elements and the four primitive qualities. He denied that material bodies were composed of various portions of earth, air, fire, and water; and that the differences between bodies arose from different proportions of the elements. And he rejected the analysis of causes that was dominant in the schools.[7]

For van Helmont, the basic element was water, fundamental in both

his metaphysics and his physics. As a metaphysical concept, water had certain similarities to unformed matter in the Aristotelian sense. Van Helmont provided curious religious-metaphysical arguments.[8] For example, Scripture declared that God created heaven and earth, and then on the first day He created light and separated the light from the darkness. On the second day God created the firmament to separate the waters above from the waters below. Nowhere does it specifically say that God created the water. Clearly, therefore, water existed prior to the creation of either life or the firmament. The waters were already included in the concept of heaven.

Thus we have an archetypal and divinely created metaphysical water, especially close to God. This water, however, had kinship to the 'lower' terrestrial water. The waters above—metaphysical—and the waters below—physical—were of essentially the same nature and van Helmont used each to prove the existence of the other. In this way he found support for the concept that water was 'elementary matter' or 'prime matter' as well as the ordinary familiar fluid subject to daily observation and experiment.

We must appreciate this dual nature and its significance in van Helmont's doctrines. As a chemist he experimented with water in various states and studied its properties. As a philosopher in the classical tradition, he had to have some metaphysical substance in which various *qualities* might inhere. Aristotle had provided such a concept with his *hyle*, or indeterminate matter, on which forms were impressed. Van Helmont expressly contradicted Aristotle, but, like so many rebels, nevertheless quietly adopted quite comparable concepts. For van Helmont water served much the same function as did the Aristotelian *hyle*, and material things were composed of water and 'seeds' (rather than 'matter' and 'forms').

In addition to primitive water van Helmont had another primary metaphysical reality, the air. The heaven which God created before the first day not only contained water but also something else which we call air, sky, or 'vital air' [*aër*, *aethera*, *vitalis aura*].[9] This vital air or vital breath clearly had neoplatonic import and relevance, as we shall see, and was especially important for the doctrine of the Archeus.

The primitive water was the source of all material things. Earth was the 'fruit' produced by the impress of 'seeds' on the water. Van Helmont referred to this process of derivation as occurring *per aquae impraegnationem*, which Chandler translated with the graphic phrase, 'a

getting of the water with childe'.[10] Seeds represented the *agents of specificity*, and conferred upon the primitive matter [water] the characteristic properties inherent in the seed. Gold, for example, consisted of water which was sixteen times as dense as ordinary water. It was the seed of the gold which had changed the water into something far different. In support of this idea he pointed out that water can become vapour which is one hundred times lighter than itself and still remain water; why, therefore, could it not also become more dense than in the ordinary liquid state?[11]

In brief, water plus a seed equals an earthy body. Van Helmont was very familiar with chemical processes involving alkalis, acids, neutralizations, distillation, salt formation. He employed the familiar Spagyric concepts of mercury, sulphur, and salt, as representing certain basic properties of agents. If particular substances, each having its own sulphur, mercury, and salt, should yield water after various chemical reactions, he considered this as proof that the original substance was derived from water. In other words, any chemical manipulation which yielded water and salt 'proved' that the original substance was 'only water'. The specificity had been provided by the original seed, and the individual properties of the substance represented the relative mercury, sulphur, and salt of those specific substances.[12]

It is in this chapter[13] that he described the famous experiment of the tree, grown in a pot for five years, After weighing the tree and the soil in the pot, before and after the experiment, he indicated that the increase in weight of the tree resulted from water only.

Van Helmont, as a biologist, devoted himself largely to problems of process and change, 'generation' and 'corruption', coming into being and passing away. Aristotle, of course, had provided a framework for approaching these, through the concepts of form and matter, the potential and the actual, and the so-called four causes—the material, efficient, formal, and final causes.

Van Helmont criticized the Aristotelian concept of efficient cause—the actual agent—because Aristotle considered it to be external to the thing in question. As an example of efficient cause, we can think of a blacksmith making a horseshoe, shaping the raw iron and making it into the form which it eventually assumes. The iron might be considered the matter, the particular shape of the shoe would be the form, the blacksmith would be the efficient cause, and the goal or purpose of the horseshoe would be its final cause. Van Helmont, thinking in biological

terms, objected to the notion that the active efficient cause should be external to the object itself. He felt that causation, in the sense of efficient causation, lay *within* the essence of the object in question rather than outside. For him, the entire efficient cause—that which brings the thing to be what it is—was internal and formed part of the *essence* of the object. Furthermore, the idea of process, the passage from potentiality to actuality and the 'realization' thereby of the inherent possibilities, all this van Helmont wanted to include as an internal essence of a thing. Of course, he apparently ignored or misinterpreted much of Aristotle, for the Aristotelian *entelechy* has a great deal in common with his own ideas. But van Helmont chose to ignore these dynamic aspects of Aristotle and to criticize the aspects which derived from inorganic activity.[14]

To indicate the inward dynamic efficient cause, internal to the object, forming its essence and making it what it is, van Helmont employed two terms but did not always clearly distinguish between them. One is the Archeus, which is the *agent* [*internum agens, efficiens, sive Archeus*].[15] Van Helmont also used the term 'seed' as the internal efficient cause, indwelling, necessary, and sufficient. As we shall see, he also conceived of 'ferment' as an active indwelling agent.

Clearly, if we regard a living creature, such as a plant, we readily appreciate that its seed contains within itself the 'power' of shaping that plant into its particular form. The seed of a carrot will clearly produce a different plant from a seed of a radish or beet. The reasons why a carrot is a carrot and not a radish lie within the confines of the seed itself, and are not imposed upon the plant by external agents. To be sure, external factors can affect the *properties* such as the size or shape, but cannot change the *essence*. Environment cannot change a carrot into a radish. The seed, essential and internal, is the immediate and efficient cause of the plant.

There are, said van Helmont, two and only two [*nec plura*] prime sources [*prima initia*] of bodies and bodily causes. There is the element of water, or the source [*initium ex quo*]; and the ferment or seminal beginning [*initium seminale*] as the agent [*per quod*].[16] From this is produced the seed and with the origin of the seed there is life. Thus the ferment apparently represents the most general agent [comparable to the Form of Aristotle] while the seed is the more specific vital agent. The question of just what was alive and what was not alive, so crucial in distinguishing between ferment and seed, raised great diffi-

culties. And so did the associated problems of spontaneous generation. Consideration of these in detail would carry us far afield.

Not only was the efficient cause internal and essential, but so too was the final cause. The idea of an external final cause, in the sense of an unmoved mover to which an object directed its activity, was abhorrent to van Helmont. Purpose or goal or direction were all intrinsic and indwelling within an agent, forming part of that principle which produces and directs successive changes of an object. Van Helmont quite failed to give Aristotle credit for the literal meaning of *entelechy*—having its goal within itself.

III

The concept of Archeus plays a central role in Helmontian doctrine. It does not constitute the vapouring of a disordered mind, nor is it a mystical conception divorced from scientific rigour or acumen. Although the concept of Archeus is foreign to our present-day thinking, it has a straightforward role in Helmont's philosophical system. But to understand this we must have sympathetic appreciation of the metaphysical problems.

The Archeus, first of all, entered into relation with the 'spiritual' properties of the universe. It directly developed from neoplatonism, which had a strong revival in the 16th century and was highly influential in England in the latter 17th century.[17] Neoplatonism emphasized the aspects of the universe that are immaterial and most real. The original formulation by Plotinus[18] embodied monism—that God, pure spirit, ineffable and beyond all description or predicates, was the Absolute One. The divine essence 'overflowed' so to speak, and differentiated. The first differentiation produced the world of intellect, the realm of Ideas or *Nous*; further differentiation brought into being souls, with varying degrees of divinity. The soul of man had a place in this realm, and so did the world-soul, or agency through which lower worlds were created. By a gradual process of further emanation, material entities came into existence, as the spiritual substance became 'thickened', so to speak, in a descending hierarchy. The completely immaterial developed or changed into extremely fine or subtle material, and then became progressively more material until we end up with

gross matter. It all represents a definite continuum, in the neoplatonic tradition.

The Archeus of van Helmont represents this spiritual realm, and thus connects up with the 'real' world of ideas, mind, spiritual substances, and ultimately God. The connection with the 'soul' of religion is quite obvious, but even more significant is the connection with the metaphysical reality of the immaterial world.

The Archeus is also a principle of *activity*. An important dispute in the 17th century concerned the metaphysical problem, what is the origin of motion? Clearly, in many material objects motion derives from pre-existing motion, as in the impetus transmitted from one moving object (say, a billiard ball) to another. But some objects—particularly living creatures—are 'self-moved'. One philosophy insists that such motion had its origin outside the body, deriving ultimately in one or another way from a spiritual principle quite distinct from inanimate matter. The alternative viewpoint, held by the classical atomists, maintained that motion represented an intrinsic property of material atoms—that to be an atom meant to be in motion. As we shall see, this was the viewpoint of the 'mechanical philosophy'. A compromise, significant in its day but now clearly seen as makeshift, was the combination of the two. God created the atoms *and* implanted motion within them. Thereafter the atoms went their own merry way. The nominal supremacy of God was thus maintained but only by a stratagem. For the mechanical philosophy the atoms in motion represented the effective area of study. But for van Helmont, or the 'spiritualist' viewpoint, the principle of motion remained at all time a property of spiritual reality.

The Archeus, then, maintained continuity with the real spiritual world. It also served as the source of activity, particularly in living beings; in this latter capacity it indicated that motion is different from matter, and that self-activity is different from derived motion. Thirdly, the Archeus represented a principle of continuity. All things are in change and flux. Appearances come into being and pass away. What is the 'permanence' which remains throughout the various transformations? One answer is the Aristotelian concept of matter as the receptacle of Forms. A 'thing' can be construed as 'forms' inhering in 'matter'. If, however, we have a more dynamic approach to reality and concern ourselves more with process than with things, we need a *subject* to underlie the various activities and to tie them together. Take,

for example, the activities of the stomach. As it digests food, it constantly enters into relations with other organs and other vital activities. We need a sort of substrate for the continuous activity, but this, obviously, must be quite different from the 'matter' or substrate of an inert object such as a stone. The Archeus represents, then, a spiritual 'matter' serving as the locus or substrate for various vital activities, to unify them, provide continuity and direction.

We have seen that van Helmont has two kinds of metaphysical reality, the water and the air. The Archeus derives from the air, while material objects derive from the water.

Archei are multiple. In the living body there are many Archei, with the principal or chief Archeus residing in the stomach. This is by no means ridiculous, as some critics would like to make out, for 'stomach' represents the principle of 'digestion', and this, for van Helmont, was the equivalent of present-day metabolism. Wherever we have vital process and change, we have activity of an Archeus. This is important in physiology, and important, as we shall see, in pathology and the conceptions of disease. With this general background, we can approach the specific discussion of the Archeus, a concept with which the modern reader has difficulty in being sympathetic.

Van Helmont did not want to associate himself with any sort of mechanistic or materialistic viewpoint. Whatever comes naturally into the world, he said, necessarily has a beginning of its motion, an activator and an internal director.[19] To appreciate this, we start with the presupposition that everything is in process. With this in mind we can understand his statement that every material object encloses within itself an 'air' which, prior to the processes of change [*generationem*], represents or foreshadows [*adumbrat*] the future development [*generationem*] as a fertile 'seed'. This air, representing the inner future development, accompanies the object to the very end—'an air which accompanies all future generative activity, as a fertile seed, internal to the object'.[20]

This air van Helmont called the Archeus, the internal efficient cause. It contained all the seminal activity and was responsible for all future activity or change or coming-into-being or ceasing-to-be that will involve the object in question.

Just as water had different degrees of compression in various material

objects, so too the air, the source of the Archeus, had different degrees of compression, responsible for the differentiation of various Archei. The primitive air varied in its degree of excellence [*in aliquibus luculentior*]. In vegetables it was somewhat compressed, and in metals, markedly compressed and homogeneous. In some objects it had the property or character [*donum*] that is called Archeus, containing the fertility of generation and of seeds. He called this the 'workman' [*faber*] which held the responsibility for all future activity, change, coming-into-being.[21]

The Archeus had, so to speak, a two-fold composition. On the one hand it was related to the vital air as its 'matter', while it also had a 'seminal image' which provided specificity and acted as an 'internal spiritual nucleus'. The Archeus, a soul-like entity, was derived from an undifferentiated air which became differentiated when it combined with an idea or spiritual centre. In the 'spiritual' realm there was thus a substrate and a differentiating agent—a sort of Matter and Form—similar to what was found in the 'material' realm. For the material world, in comparable fashion, the archetypal or elemental water served as the substrate and became differentiated by 'ferments' or 'seeds'. Van Helmont made it very clear that any concrete visible seed was only the husk [*siliqua*] of the spiritual nucleus making up the Archeus.

To the basic philosophic problems—how does the world come into being? how can we explain permanence and change?—van Helmont gave an answer that related to the classical Platonic tradition. There are basic irreducible qualitative aspects, which we can call 'ideas' or 'forms' or 'ferments'; and these act upon a metaphysical substrate which, for van Helmont, consists of water and air. The qualitative aspects are all dynamic, propulsive, with inward driving force. They represent the efficient cause of things and are of spiritual nature. The Archeus or soul-like entity has persistence amid change and is responsible for the changes which unroll in regular and predictable fashion in the material world.

At the same time van Helmont busied himself with the science of chemistry. He made extensive experimental investigations with techniques that were good for their day, and he also wrestled with the practical problems of disease. He used his metaphysical background to explain the phenomena of medicine. His metaphysics served as his 'first principles' and the 'scientific' basis from which he could make appropriate inferences.

The concept of ferments is extremely significant in the doctrines of van Helmont, for it has to do with the very essence of life and the inter-relations between the living and the non-living. A familiar example is the manufacture of beer, a process of fermentation whereby a particular substance acts upon a substrate and produces a transformation. The ferment concerned in the making of beer was not alive, since it was not associated with an Archeus. But the same process of fermentation—or perhaps we should say, an analogous process—was responsible for vital phenomena.

Material objects were formed from the primitive (metaphysical) water when acted upon by an appropriate ferment. We see this process in living organisms. In the sea, fish were formed of water and nourished by water, and the transformation of sea water into the various parts of the fish—their flesh or, in crustaceans, the shells—occurred through ferments.[22]

Van Helmont thought that ferments from putrescent material could bring forth forms of life. This was the doctrine of spontaneous generation, which had important theoretical implications. The various inhabitants of marshy water, he thought—frogs, snails, and the like—arose from the ferments of putrescent objects. This also occurred with other forms of life, from insects to mammals, and in many kinds of decaying matter.[23] For it to take place there was required a substrate and a ferment. The latter could derive from any foul object. Van Helmont gave the example of a man's dirty shirt, squeezed over the mouth of a vessel which contained some wheat. In a short space of time the character of the wheat would change, he thought, and it would be transformed into mice.

Generation could take place in two different ways. One was by sexual reproduction; the other was by the action of ferments derived from the corrupting matter and acting on a suitable material. He thought that many forms of life—lice and flies, gnats and worms—could similarly arise from ferments coming from our bodies. The living forms produced in this fashion, such as the mice described above, would come into being already grown, and would not go through a stage of infancy.

There are, then, two types of ferments. One of them was not directly alive, but it could accomplish biological transformation and produce essential changes in living creatures. The second type of ferment, however, might become transformed into a living object. It

contained in itself a flowing air, the seminal Archeus, whose flow brought life [*qui suo fluxu in animam viventem aspirat*].[24] Once it was associated with an Archeus, the ferment could reproduce sexually and produce seed of the ordinary type.

The relationships between seed and ferment and Archeus are a little obscure. In the plant world, reproduction was obviously carried out by seeds, while in the animal world the male generative principle was designated by the Latin word for seed. A seed, he declared, was a substance in which an Archeus is present [*substantia, in qua iam Archeus inest*][25] and this Archeus is a 'spiritual Gas' containing in itself a ferment, the image of the thing, and especially the 'disposing knowledge' [*notitiam dispositivam*] of things to be done. This passage suggests that the seed contains an Archeus, which contains a ferment. However, in a preceding paragraph[26] he has declared that one type of ferment contains in itself the flowing air, the seminal Archeus. We remain somewhat uncertain whether the Archeus contains the ferment or the ferment contains the Archeus. The most likely interpretation will consider them as intimately associated in living entities, and we should not use a spatial metaphor of containment.

There is a close relationship between ferment and odour. Various excrements and putrefactions seem to give rise to spontaneous generation and they all have in common a bad odour. It seemed natural to consider the odour related to the active principle of the ferment, and only a short further step to consider the odour the active agent. Certainly, bad odours could produce extensive physiological effect, such as headaches, vomiting, nausea, and others; and, on the other hand, perfumes could have strong effects on the mind.[27] Many diseases, in addition, seem to be related to odours.

Perceptible odours often indicated physiological activity. Van Helmont was willing to assume the existence of imperceptible odours, to generalize widely, and to have fermental action carried out by means of odours, whether perceptible or not. In fact, he insisted that many medicines, for example, exert their effect by means of an odour, and that the smell of the medicine could carry its curative virtue. He believed that erysipelas was cured by the *odour* of a towel dipped into the blood of a hare, then allowed to dry, and then bound on.[28] This concept offers an interesting comparison with the powder of sympathy, discussed later. (pp. 140ff.[3]).

The reasoning may be summarized: odours exert physiological

effects. Ferments exert chemical and physiological effects. Odours are often closely associated with ferments and fermentation. Therefore, ferments act by way of their odours. And, as a final generalization, all ferments act by odours, whether these are perceptible or not.

Van Helmont believed that ferments impressed their own alien character upon the atoms. For fermental action to take place the substances must be broken up into their finest or smallest components.[29] Different parts of the body all had their ferments, as we shall see when we discuss digestion.

Through his concept of ferments van Helmont was able to connect the living with the non-living, and place them both under a common rubric. Some ferments were not alive, i.e., they did not have an Archeus. On the other hand, other essentially similar agents were alive, within seeds or bodily tissues. Furthermore, the distance between the living and the non-living was extraordinarily slight, since, through spontaneous generation, the non-living could acquire an Archeus and become living. His metaphysics enabled him to bring together the phenomena of the living and the non-living into a single broad unity.

IV

The process of digestion, a major component in Helmontian theory, has received considerable attention from historians.[30] Van Helmont, in his analysis,[31] presented both a negative and a positive aspect. The negative aspect was a sharp criticism of the earlier doctrines, performed with considerable logical acumen. The positive aspect presented his own views of fermentation.

His critical artillery he directed against the concept that heat was the effective agent in digestion. The idea itself was not implausible. Heat *in vitro* will bring about substantial changes in foods and cooking will induce profound alterations. Heat was intimately connected with life, and the bodily organs were actually hot. Since digestion *in vivo* produced profound changes in the foods, it seemed not unreasonable that this bodily process took place by a sort of cooking. The reasonable conclusion indicated that heat was the effective digesting agent. The heat, incidentally, was considered to exist in various 'degrees'.

In criticism of all this van Helmont pointed to various phenomena

which indicated the *differences* between heat *in vitro* and digestive agents *in vivo*. A dog can digest bones, but fire cannot change bones into chyle. Again, cold-blooded animals, such as fish, can digest food, but do not have any 'actual' heat [*nullus actu calor est*].[32] It is absurd, he thought, to explain this by 'potential heat'. He could not accept the idea of a heat which was not actually hot but merely a 'virtual power'.[33] Furthermore, the food, as it left the stomach, is vastly different from any change achieved by heat. In hot-blooded animals heat, although it accompanies digestion, is not the effective agent.

He offered many other striking examples. The digestion of a given food will vary in different animals—man, dog, horse, or fish—and what produces different effects must have different efficient causes. And various animals can digest substances indigestible to others. Birds, for example, can digest glass, bricks, chalk, and the like. Furthermore, if heat were the active agent, a person who was ill with a burning fever should have a more powerful digestion than a healthy person. But this is contrary to observed fact. Something else besides heat is the cause, the *causa continens*, of digestion.

The digestive agent or ferment is related to acidity but differs from acid as such. He knew, for example, that birds have an intense acidity of their digestive tract. He found this out when, as a child, he would put out his tongue, and a sparrow would try to swallow it. He thereby perceived, with his tongue, the great acidity of the sparrow's throat.[34]

The digestive ferment, however, is quite different from the acid substances found elsewhere. Vinegar and lemon juice, for example, will not ferment flour. Again, if the ferment consisted only in acidity, various acids should ferment bread and digest meats. But this does not happen.[35]

A digestive ferment is something closely related to acid, but having a specific vital property. He called it 'an essential property, consisting in a certain vital acidity, powerful in bringing about transformations, and possessed of a specific property'.[36] The 'vital acid ferment' is thoroughly different from acid as such. The effective agent is the ferment, and van Helmont had connected up empirically some data of digestion with the concept of ferments as a general chemical activity.

These studies on digestion provide interesting examples of scientific methodology. Van Helmont was quite critical in regard to the abstraction 'heat'. The word had to cover too many different activities and concepts. We have already seen that Riverius was troubled because,

according to his doctrines, different aspects of nutrition were all performed by the Hot and the Dry. Riverius had realized that a single term could not satisfactorily cover a variety of activities, unless there were present some further differential factor. But he made no attempt to find the differential factors. Instead, he adopted the concept of heat in various 'degrees', referring not to any scale of temperature but rather to arbitrary properties, assigned on a more or less *ad hoc* basis.

Heat had a central place in Galenic science. Surely the followers of Galen had realized that food heat-treated *in vitro* was not identical with chyle produced in the stomach. Multhauf[37] has emphasized that van Helmont took literally the statements of the Schools that had been intended as illustrative or analogous. But Galenists were concerned with the similarities in nature, not with the differences, and finding similarities among phenomena would strengthen the position and stability of the doctrines. This is reasoning by analogy, and in analogical reasoning it is important to stress these similarities and to ignore differences. I do not believe that there was any deliberate intent to ignore differences. Rather, they seemed of no importance and tended to make no impression. They fell upon the blind spot of the intellectual eye.

Van Helmont eliminated this blind spot by attending to the differences among phenomena. Previously the various exceptions, contradictions and difficulties simply made no impression, but after his critique these same exceptions and contradictions took on new significance and indicated a change in mental attitude. When, in dealing with analogies, we attend to exceptions rather than similarities, theories may appear very shallow, founded on only superficial resemblances. An important feature of 17th century scientific progress was the attention paid to the exceptions rather than the similarities.

But when van Helmont eliminated one blind spot, he substituted another. He was quite critical in regard to the term 'heat'. He made careful discriminations which showed that the concept as then understood was not capable of exerting explanatory force. He substituted a different concept, namely that of ferments, with which he could explain a great many different phenomena and relate them one to the other. Yet, in so doing, he extended the concept of ferments far beyond the available evidence. Despite this he was able to bring a new unity into phenomena, and reconstruct the fragments which his criticism of heat had left.

The Galenic concepts involved a three-fold digestion, the first of which, taking place in the stomach and intestines, produced chyle which was absorbed by the veins and carried to the liver. The second digestion occurred in the liver, where the thin parts of the chyle were separated off and excreted in the urine, while the thicker part was transformed into blood. This second digestion began in the body of the liver and terminated in the ultimate branches of the vena cava. The third digestion comprised the process whereby blood was changed into nutriment for the solid parts. As we have seen, this involved a preliminary change into the secondary humours, and comprised four different grades of affinity, the last of which was the actual assimilation. We have seen how Riverius tried to implicate the Hot and the Dry to explain this process.

Van Helmont divided the total process of digestion into six steps. The term 'digestion' had a very broad connotation, for it included all stages between the ingestion of food and the metabolic activities of the tissues—or, in Helmont's terms, the assimilation to the parts—and was carried out by ferments. The first step took place in the cavity of the stomach where the first ferment acted, to draw the food and drink into a 'cream'. The ferment of the stomach, van Helmont believed, came from the spleen. [A connexion between the spleen and the stomach, through the vasa breva, was a firm aspect of Galenic physiology.] For van Helmont the spleen and the stomach together made a single 'duumvirate', and the two organs together had special importance in bodily physiology.

Because the liver was a solid organ with only relatively few veins, all the digested material absorbed from the intestine, together with the fluid which became urine, would necessarily pass swiftly through it, so swiftly that the chyle could not be changed into venous blood. The process of sanguification must take place, therefore, not in the liver but in the mesenteric veins.[38] The change of chyle into blood was a process more complex than the change of food into chyle and required different ferments. Furthermore, it could take place only if the many narrow channels were sufficient in number to provide an adequate capacity. This passage has considerable significance in view of Boerhaave's later emphasis, that changes take place in minute vessels, the equivalent of our cells today.[39] Van Helmont, it would seem, believed that fine changes required minute 'laboratories' for their accomplishment.

The mesenteric veins he called the 'stomach of the liver', that is, the

place where the transformation took place. This stomach or kitchen was under the direct supervision of the liver, which provided the necessary ferment. In the first digestion, the stomach was the receptacle and the ferment furnished by the spleen. In the second digestion the small intestine was the 'stomach' and the ferment furnished by the bile, which produced a break-down and separation of the watery parts[40]. The veins of the mesentery were the 'stomach' of the third digestion, with the ferment furnished by the liver.

In the duodenum, van Helmont recognized, acid was neutralized. The urine—the waste salt fluid included in what he called 'latex'—was eliminated. This took place by an 'attraction' exerted by the kidneys, across the liver. The Galenic idea of 'attraction' was still dominant and it is interesting to see this 'faculty' type of explanation used side by side with the concept of ferments. Van Helmont described many other specific physiological aspects of the digestive process which we cannot here take up, such as absorption through the intestines, or the formation of faeces.

Van Helmont provided a more detailed analysis than did his predecessors. Besides introducing the concept of ferments as the active agents, he discriminated three steps in the formation of blood: the digestion in the stomach; the neutralization in the small intestine, through the action of the bile, with further separation of fluids from solids and a provision for eliminating the urine by 'attraction'; and the transformation of chyle into blood, with the locus in the mesenteric veins, and the effective ferments supplied by the liver.

In discussing the fourth and fifth digestions, which concern the transformation of venous blood into whole blood, van Helmont revealed certain basic confusions. He held to the Galenic concepts of anatomy, with the passage of blood from the right side of the heart into the left, through pores in the septum. The fourth digestion was the process whereby venous blood became arterial. In this process, which occurred in the heart, the colour of the blood changed, and the thicker venous blood became more volatile.[41] In a previous section, 'The Blas of Man', van Helmont had discussed the change from venous to arterial blood.[42] The change in appearance of the blood was one, step, but in addition the arterial blood was furnished with a vital spirit produced by a ferment in the left heart.[43] The arterial blood had the function of distributing this spirit.

The change in appearance between the venous and the arterial blood

he did not directly associate with the 'vital spirits' or oxygenation. He believed that two separate processes were taking place. For our purposes the significant feature is his claim that the vital spirits were produced by a ferment in the left heart. The air, he thought, was sucked into the blood through the arterial vein and the venal artery, and transported to the heart where it could receive a ferment. The air could make the blood more volatile, for heat without air would render the blood too thick. The inspired air counteracted the body heat. As evidence he pointed out that fishes, which lack body heat, do not breathe air—there was no heat to thicken the blood and hence no need for respiration to keep the blood volatile.[44]

The sixth digestion van Helmont described as the process whereby the parts of the body were nourished. Each part had its own 'stomach', or 'kitchen'. Van Helmont rejected the description of the secondary humours and their transformation, described previously, but tried to attribute the activities to ferments, each specific for its function. Unfortunately, for the sixth digestion he did not go into as much detail as he did for the first three.

The Galenic physician and van Helmont were talking about the same phenomena, but where the Galenists were content with a 'faculty' as explanatory mechanism, van Helmont provided a more specific explanation, namely, the concept of ferments. However, this was still explanation on a verbal level, for only in a small area, such as gastric digestion, could he provide empirical evidence for ferments. For other functions, in other parts of the body, he extrapolated without providing concrete evidence. In large part his 'ferment' was simply a 'faculty' in a new dress. An important difference, however, was the implication that the phenomena so designated were similar to the phenomena elsewhere which *could* be explained by concrete and demonstrable chemical features.

V

A major philosophic and scientific problem concerns the phenomena of change. How do changes come about? The Aristotelian explanation rested on the doctrine of Forms. Forms may succeed each other in the substrate. I like to give the analogy of a moving picture, where there is a 'fade-out' and a 'fade-in' occurring in the screen directly in front

of us, and as a result we have a complete change-over in what we are looking at. The old form fades out as the new one takes over.

We can distinguish genuine transformation from any mere putting together, where components preserve their own essence. For example, mixing sugar and water, or sand and sugar, produces an apparent change but does not affect the essential nature of either. But other changes represent a true alteration—e.g., subjecting sugar to a hot fire. This produces a destruction of properties and the coming into being of new properties.

In real change there is a transmutation but also a persistence. Digestion offers the best example. In digestion we have a transformation of properties. We start with food, which after a series of intermediate steps ends up as flesh. This, of course, is not the ultimate step, for flesh will die and become 'corrupted'. In the ceaseless succession of changes, some parts of what we start with are 'utilized' or, differently phrased, take an intrinsic role in the transformation. Other portions of our original material cannot be utilized and remain as dregs. What cannot be utilized in digestion (understood in its broad sense of metabolic activity) must be excreted. Paracelsus held that much of disease could be traced to material that should have been excreted but was not—the doctrine of tartar.[45] In an ideal state all transformations are complete, with no residue to produce difficulty.

To explain incomplete utilization van Helmont invoked the concept of a 'middle life'. The middle life consisted of certain persisting qualities which were an intrinsic part of the substance in question. While the substance as an entity might disintegrate, certain properties would persist and be carried over unchanged into some other being which had absorbed—eaten, or otherwise acquired—the substance in question. For example, the urine of a nursing infant might show qualities of the food which the nurse had eaten. The infant's urine would show an odour if the nurse had eaten asparagus. This would derive from the middle life of the vegetable.[46] Or if, for example, a hog ate fish, the meat of the hog would have the savour of fish, but nevertheless remained hog. The residue of the fish was its 'middle life'.

Certain properties persisted and remained operative, even though the 'context'—the other properties and relationships—was transmuted, corrupted, or otherwise changed. In van Helmont's terminology, the properties of the middle life came under the 'dominion' of the Archeus whose ferment was acting on the material in question.

This concept of a middle life was a way of preserving a certain continuity of qualities or properties. Within an organismic whole, certain parts might disintegrate but other parts remained functionally active. In a sense van Helmont was adopting an atomistic schema. In atomism certain elements persist; change takes place only in their combination and arrangements. Van Helmont, not a classical atomist, believed in persisting *clusters or groups of qualities*, which could remain even though their original parent form might disintegrate or otherwise alter. The smell derived from asparagus would persist in the body of the person who ate the vegetable, but could also be transmitted quite unchanged to the nursing infant. This persisting identity of a property —all on a qualitative basis—indicates the 'middle life'. This middle life was van Helmont's equivalent of the 'occult qualities' against which, as we shall see, Robert Boyle contended. Medicines, after ingestion, can act favourably on the human body only because of their middle life. And poisons acted in a comparable fashion but, of course, their activity was unfavourable.

Van Helmont tied together theology and the doctrines of a middle life. In a state of bliss, that is, in the Garden of Eden before the fall, the human Archeus was able completely to 'subdue' all other Archeuses without any residue, without any poisonous or harmful leftovers. The Archeus could readily overcome all 'impression' of the middle life. But once man was cast out of Eden, the earth produced 'thistles and thorns', a metaphorical expression indicating residual matter that could act in a harmful fashion. Once man had lost the state of perfection he was no longer able completely to assimilate foreign substances but these could preserve some residua, much of which might be harmful. The residual powers of the middle life, whether in food or in excreta, were the origin of almost all illnesses, and to this degree were for us as thistles and thorns.[47]

Biological events, for van Helmont, fell into three subdivisions which he indicated as the 'first life', the 'middle life', and the 'last life'. In a plant, for example, the first life was the seed; in the middle life there was growth and maturity; and when the plant died it had obviously reached its last life. But the last life of the plant, with the production of the seed, was the first life of the new organism that would arise from the seed. If the grain was harvested and eaten, this also was the last life. However, when the last life of the grain died, it became part of the middle life of the man who ate it. Biological transformations

thus represented a shuttling back and forth of first, middle, and last life, according to the environment and the course of events. The mature wheat was the end of the wheat seed but only the middle life for the nutrition of the animal. The animal, in turn, could be the middle life if eaten by another animal.[48]

An admirable example of van Helmont's doctrine, and its relations to magic and the reasoning process involved therein, we find in his cure for warts.[49] To remove warts he recommended that we should cut an apple and rub the inner cut surfaces on the wart. The pieces were then tied together and the apple allowed to putrefy. When putrefaction became completed, the wart would have disappeared. He offered this explanation of the mechanism: The 'impression' of the wart had been 'attached' [*conglutinata*] to the apple. As the apple reached its last life and perished, so also perished the last life of the wart. There was an extinction of both the apple and the wart, whose last lives had been intermingled by a sort of sympathetic action. However, if the apple, before it putrefied, was eaten by an animal, the wart would not disappear, for in this case there was a 'retrocession' to the middle life. In the stomach the Archeus would, in a sense, maintain the life of the apple, by retrocession to its middle life. Consequently, the life of the apple would not be 'extinguished', and neither would the wart. But if death occurred through putrefaction there was no preservation of the middle life and hence the apple and the wart would perish together. Thus, in some way, the impression of the wart became transferred into the apple and both apple and wart would either perish together or 'survive' together.

The concept of first, middle, and last life van Helmont applied even to minerals. Indeed, he applied these ideas to anything that had an interrelation of parts. It was a sort of qualitative atomism that reminds us somewhat of the teachings of Anaxagoras, yet maintained the sense of pattern and relationship.

The doctrine of the middle life had great relevance in disease, for it was one way of dealing with the complex phenomena of biological change. A congeries of qualities and properties formed a sort of unit that left the dominion of one Archeus and came under the sway of another Archeus—that is, in more familiar terms, passing from one biological system to another.

When, say, specific poisons acted on the body, there was a combat between the properties of the 'middle life' carried over by the poison and the strength of the Archeus. If the Archeus was capable of subduing the invader, health would result. Disease was the furious raging activity of the Archeus, trying to expel or overcome the agent. We can compare this with the Galenic view of health consisting in proper proportion, and a disordered proportion representing disease. We shall note this again in the doctrines of Sydenham. Van Helmont had utilized this concept but placed it in an animistic framework.

A complex of qualities, such as the aromatic properties of cinnamon or cloves, or the laxative properties of certain plants, represented a middle life which ordinarily would be 'subdued' by the Archeus. But if these complex qualities were sufficiently strong they acted by a 'luminous and abstract force attached to its solid substance' [*per vim luminarem, & abstractam, concreto suo illigatam*] and they 'impress' [*imprimere*] their activities on the vital form.[50]

The schema of van Helmont emphasized immaterial forces—*something* which acted but which could not be directly grasped by the senses. It could be understood only by its effects. Light was such an entity and so were thought, ideas, the mind and its workings. This immaterial force or activity also applied to the concrete problems of disease. Disease resulted when the Archeus could not properly handle the impressions, could not properly subdue the middle life, nor properly direct the desired course.

He gave the example of the bite of a mad dog which, he believed, might transfer a seal-like 'idea' [*sigillarem fantasiam*] which would convert the sufferer into its own character and convey its own 'luminous property' [*luminarem proprietatem*] to produce hydrophobia. This took place through the Archeus. There was no need to dispute whether the Archeus would create [*fabricaret*] the virulent 'image', or whether the image would be impressed upon it. The important thing was, that under the influence of these 'luminous actions' there was a conjunction. The *occasional cause* [the bite] and the *efficient cause* [the Archeus] became conjoined and the two acted as one.[51]

The concept involved in the bite of a dog would also apply to other diseases, such as a cancer. The bite of the dog made the Archeus raging mad, and a similar virulent 'light' [*lumen namque formale virulentum*] in a cancer induced a rage in the Archeus, drove him into 'fury' [*furores*] whereby the flesh was infected.

How this 'light'—or immaterial agent—affected the Archeus, van Helmont did not try to say. Nevertheless, many kinds of poisons acted on the body in different ways, sometimes even multiplying within the body, and serving as agents which acted on the Archeus. This interaction, between some quality or 'light' or idea and the immaterial Archeus, represented the 'immediate action of forms on forms'. There was an interpenetration and union. Van Helmont spoke of the formal light of poisons which penetrated the vital light [*Ast lumina formalia venenorum, penetrant lumen vitale*].[52]

The way in which an idea could produce a material effect was quite familiar in the concept of the mother's imagination disturbing or impairing the foetus which she carried. In discussing the effects of emotion, van Helmont spoke of grief or sadness bringing forth the active idea and this, in a pregnant woman, could change the foetus into a monster. The significant feature, of course, was the effect of an idea or immaterial entity on the Archeus, and through the Archeus on the foetus. The idea became translated into concrete effects.[53]

All diseases were not hereditary, but only those where an idea had affected the *insitus Archeus* concerned with generation. The idea was impressed on the seminal spirit. The disease, concealed and impressed in the first life of the seed, lay dormant, and reached maturity later. Only those diseases were transferred from the parents to the children whose 'ideas' contaminated the 'visceral Archeus in the parents' (that is, affected the Archeus controlling the seed).[54]

I wish to emphasize the concept of an active immaterial agent. This connects up the concrete manifestations of disease with the ideal world of the neoplatonic tradition. Ideas—immaterial—can act on material things. Light served as a good illustration of an immaterial entity which produces a material effect. For the immaterial to act there must be intermediaries, and the Archeus, in a sense, was the intermediary between the world of pure spirit and the material world of observable phenomena. Van Helmont realized the large number of problems involved in trying to explain in this way the action of pharmacologic agents, for example, or infectious processes. He discussed at some length the various possibilities, but detailed discussion of these would carry us far afield.[55]

The Archeus served as a unifying 'locus' or a logical *subject* for the alterations that occurred. Van Helmont could see the physical effects of disease but not the intermediate steps. He observed functional changes

but could not find physical agents to correlate with them. To unify the series of changes, a construct such as the Archeus was logically satisfying. It represented an entity which could serve as an explanation —that is, a reference point for observed phenomena. It was part of the search for first principles.

Diseases, then, stemmed directly from the influences acting on the Archeus, and these were, at least in part, 'ideas'. In part they represented seeds or ferments. At this point the concept of Blas might be mentioned, a force from the stars which communicated motion. There was a Blas of stars and a Blas of man and van Helmont spoke about the subservience of man to the stars—problems of free will not relevant to our problems here. In the book, *Origin of a Disease Image*, he indicated some relationships between the Blas and light, as well as spiritual astral images in the air, which may excite the Blas according to the imaginal properties. The concept got rather murky, but it concerned the relationship between ideas, images, motive power and activity, whether cosmic or Archeal. He said specifically, 'In the stars there is a power (*vis*) of creating (*fabricandi*) the idea of active light (*motivi*) which is the beginning of early motion, making whatever is required for performance.'[56] Force, light, activity, spiritual forces, ideas, all were intermingled.

Diseases, then, arose directly from the influences which acted on the Archeus. Health and disease were closely similar and the mechanisms that preserved health would, if perverted, produce disease. The Archeus was responsible for health and also for disease. Because of this we must place great significance in van Helmont's statement that in the cure of disease there was little virtue in removing the 'occasional causes'—i.e., the actual precipitating agent—unless the Archeus was first pacified (*pacato*).[57] This was Helmont's way of saying that diseases have their basic root in the inmost character of the organism, and not in the environmental factors which happened to set the diseases into activity.

VI

Van Helmont combined many distinct aspects. He was a biologist, a chemist, an experimentalist, a neoplatonic philosopher, a mystic, and, above all, a physician who tried to establish coherent doctrines re-

garding health.[58] From our viewpoint he combined incompatibles. We today feel an incongruity between chemistry and neoplatonism, for example, between mysticism and experimentation. But van Helmont was not aware of any inconsistency and indeed, from his viewpoint, none existed. 'Philosophy' still embraced all knowledge and he was a philosopher. We cannot evaluate his teachings unless we adopt his point of view. Unless we have some sympathetic insight, his work seems quite nonsensical, and historians without such sympathy are apt to pronounce unflattering judgments.[59]

Van Helmont, despite the variability of his writings, has a fairly well rounded system. The central feature—if we can speak thus—is the Archeus, which represents in part a philosophic term, of neoplatonic significance and mystical import; and in part a physiological concept. The Archeus, in a sense, points up the crucial role of *spirit* in 17th and 18th century biology. 'Spirit' must perform double duty. It is non-material, but it is *substance* [i.e., the 'really real'], and this touches the essence of the entire Platonic tradition. The Archeus, however, performs physiological functions that are obviously material. Van Helmont tried to achieve some consistency. The Archeus, as a spiritual agency, entered into relations with forces that were either nonmaterial or so finely material that the difference was not important. Hence the emphasis on the ideas and images getting impressed on the Archeus; hence the emphasis on 'light' and 'luminous' force, and on 'form', with its Aristotelian overtones.

But the Archeus was also the agent which brought about material physiological activities that could be observed and measured. Quite manifestly, physiological actions dealt with matter, often extremely fine and subtle. Neoplatonism had introduced the concept of extremely subtle matter, a sort of 'condensation' of the immaterial, that could bridge the gap between the immaterial and material, and could progress to the existence of coarser matter. The junction of the immaterial and the finely material we can see, for example, in Helmont's views on odours and on ferments, and the 'real' seed of which the visible seed was the husk. All of these different relationships centre around the concept of the Archeus.

As we look back, the road to progress appears clear: eliminate the concept of immaterial reality—at least, so far as science is concerned. This we can readily do if we adopt (for science, but not for religion) a point of view which deals only with matter—sometimes fabulously

fine and subtle (as that which is involved in nerve action), at other times quite crude, but always material. In the mid-17th century, however, this road was by no means clearly marked out. It became clear only as other philosophers, scientists, and physicians wrestled with the problems that crowded in upon them.

BOYLE

Robert Boyle stands in marked contrast to the preceding writers. In metaphysics he embraced the corpuscular philosophy; in methodology he was an empiricist and an excellent experimentalist. Like van Helmont, Boyle too wanted to get away from the teachings of Aristotle, but he took the road that led quite directly to our modern conceptions of science.

Robert Boyle exerted great influence upon 17th century medicine. He was not a physician in the strict sense, for he did not have a formal medical education. But he had a keen interest in both clinical and experimental medicine and he proved quite influential in shaping medical progress. Indeed, the writings of Boyle provide one of the best insights into the changes which medicine underwent in this century.

The study of Boyle permits us to glimpse not only a first-class investigator[1] but one deeply concerned with metaphysics and its relationship to science and religion.[2] In addition, his writings throw much light on what we may today call the logic of science, e.g., such problems as the validity of evidence, the nature of scientific methodology, the meaning of causation. He did not deliberately or systematically expound a medical philosophy, but his vast (and unhappily prolix) writings contained ideas which imply it.

The previous chapters have discussed two major philosophic positions of 17th century medicine. The Aristotelian philosophy, reflected in Galen, was dominant, while the Spagyric tradition combined an empirical chemical approach with a neoplatonic metaphysics. Boyle, with his corpuscularian position, provided a quite different metaphysical basis, derived from a different Greek philosophy, the atomism of Leucippus, Democritus, and Lucretius.

Greek atomism held that the universe was constituted by small indivisible particles, varying in size and shape, but endowed with motion

and moving ceaselessly in a void. All bodies were composed of atoms whose combination and recombination explained all change, all generation and destruction. Atoms were 'material'. Reality comprised only the moving atoms and the void, with no other existent, no spiritual or immaterial entity. Where other philosophies invoked an immaterial reality, however designated (e.g., God), the atomists invoked only atoms of particular fineness and subtlety.

To explain through atomism the observable physical phenomena required extensive elaboration of doctrine. To explain the nature of life and physiological processes, and mental and intellectual activities, required further elaboration. The detailed history of atomism has engaged the attention of many historians, and only a few of the major secondary sources can be here indicated.[3]

In a loose sense the 'scientific revolution' was the victory of this corpuscular doctrine (appropriately modified) over the dominant Aristotelian philosophy. For our purposes the term 'corpuscular philosophy' is definitely preferable to 'atomism'. The philosophy of Descartes provided a form of corpuscular metaphysics that was not atomistic but held rather to the unlimited divisibility of particles, and a plenum rather than a void. The revival of classical atomism was due largely to the French cleric, Pierre Gassendi. Gassendi's teachings, expounded by the English physician, Charleton,[4] were particularly important in England, and have special relevance to Robert Boyle's metaphysics.

In the three competing philosophies we have a clear-cut opposition of basic principles. For Riverius the basic realities were Matter and Form. Matter was indeterminate but took on specific qualities and properties under the impress of the Forms. Qualities enjoyed prime status. The four elementary substances, manifesting the primary qualities, represented the actual building blocks of complex bodies, and bodies in combination could become invested with new Forms. The specificity of complex bodies derived from the Forms which impressed themselves thereon.

For van Helmont the primitive realities were water and the ferments. The ferments, acting on the metaphysical water, produced the concrete bodies and conveyed specificity in both form and function. The functional aspects of organisms, their Archei and the inner activities that informed the visible seeds—all real—were not 'material'. Real but immaterial existents formed the essence of the universe.

On the other hand, for the corpuscularians the realities were matter and motion. Although classical atomism was entirely consistent with atheism, the 17th century corpuscularians very carefully insisted on the existence, the glory and the power of God. Gassendi had already shown the way whereby atomism could be made to harmonize with the existence of God and God's providence, and Boyle followed the path that was already thus laid out. God, said Boyle,[5] 'when he first formed the universal and undistinguished matter into the world, put its parts into various motions, whereby they were necessarily divided into numberless portions of differing bulks, figures, and situations in respect of each other; and that by his infinite wisdom and power, he did so guide and over-rule the motions of these parts at the beginning of things, as that . . . they were finally disposed into that beautiful and orderly frame, we call the world.'[6]

God created the world by creating atomic particles. God impressed motion on the atoms, and the different properties of created bodies owe their character and specificity to the particular motions which had their origin in God's providence. Motions differed in various ways, as we shall see.

Specific motions acting on atoms were the corpuscularian equivalent of van Helmont's specific ferments acting on water. This, in turn, was the equivalent of substantial Forms acting on Matter, which Galenic medicine had derived from Aristotle.

But the similarity between the competing doctrines must not blind us to the differences. To discuss these differences within a reasonable compass I shall consider Boyle's teachings under a few selected aspects.

II

As a scientist Boyle felt quite at home in the inorganic world of physics and chemistry, much less at home in the realm of biology. He framed his doctrines principally from the data of the inorganic world and then applied his concepts to the living. On the other hand, van Helmont was primarily a biologist who paid attention particularly to the phenomena of the living and who handled the inorganic world with much less assurance.

The corpuscular philosophy was admirably suited for explaining

the inorganic world. Boyle provided an epitome in the *Origin of Forms and Qualities*. Motion and matter are distinct. Motion comes from God, the First Cause. God did, 'by establishing the laws of motion among bodies, and by guiding the first motions of the small parts of matter, bring them to convene after the manner requisite to compose the world'.[7] The essential properties of material particles (atoms) are magnitude (or size), shape and either motion or rest.[8] Qualities, far from having primary metaphysical status, or having existence in their own right, are only secondary to the truly primary properties of matter.

The distinction between 'primary' and 'secondary' is quite fundamental. 'Secondary' represents that which is not self-subsistent but derives from something which is (both logically and metaphysically) more significant, more real. The 'secondary' is derivative. The 'primary', on the other hand, does not depend on anything else, God excepted, and is ultimate or self-subsistent.

The atoms exhibit certain properties which are ultimate or primary. Boyle did not always adhere to the same terms but made the list sometimes a little longer, sometimes a little shorter. Generally the primary properties of material particles were considered to be 'bulk' and 'figure'—i.e., size and shape—and either motion or rest. But size, shape, and motion were not enough to explain the world. Boyle had to invoke another property which is really the most important of all, namely the *arrangement* or *pattern*. Boyle used many terms such as 'posture' or 'order' or 'a certain disposition or contrivance of parts in the whole, which we may call the texture of it'.[9] Indeed, 'texture' is perhaps the favourite word, used repeatedly, and always in the sense of pattern or arrangement. These were Boyle's primary qualities, that were truly real.

And what of the 'qualities' in the ordinary sense, the qualities and forms that loomed so large in the Aristotelian metaphysics? What of the properties of objects that man could perceive through his senses —colour and shape, sounds or odours or tastes? 'Men have been induced to frame a long catalogue of such things, as, for their relating to our senses, we call sensible qualities.' Men are 'apt to imagine that these sensible qualities are real beings in the objects they denominate, and have the faculty or power to work such and such things'.[10] But all this is false, for 'there is in the body [i.e., physical objects] to which these sensible qualities are attributed, nothing of real and physical but the size, shape,

and motion or rest, of its component particles, together with that texture of the whole'. When a pin, stuck into the finger, causes pain, that pain is not in the pin. No more is any other quality, like 'roundness' or 'white'. The only qualities that are really existent are those which define the atoms—size, shape, motion, and pattern or texture.

The universe, for Boyle, resembled a clockwork mechanism whose functioning depended not merely on the existence of parts, but the proper order of these parts which must stand in a correct particular relationship one to the other. The term 'texture' covered the entire concept of 'relationship'. A single external force could produce different effects on two bodies if their textures were different. That the sun will melt wax but will harden clay were long familiar examples, but even more significant examples came from embryology. Boyle pointed out that two eggs, before incubation, might be indistinguishable. Yet one, prolific, will hatch out a chick; the other, barren, will yield only a 'putrid and stinking substance'. These two eggs 'differ in the internal disposition of their parts'.[11] Boyle thus offered the explanation that the two eggs differed in the arrangement of their component particles. But *what* the difference was, *in what way* the arrangements differed, Boyle had no idea, nor did he have any way of finding out. He was content merely to implicate 'texture' as the principle of differentiation, or discrimination, without further specification. 'Texture' came to be a cover-all much like 'form' or 'ferment'.

With considerable ingenuity and subtlety, Boyle examined various possible objections, noted various apparently special cases. Always he returned to the primacy of matter and motion: perceptible qualities depended on the size, shape, motion, and arrangement of the component parts. The world and its qualities and its activities are only 'the effects of the often mentioned catholick affections of matter, and deducible from the size, shape, motion (or rest) posture, [punctuation, *sic*] order and the resulting texture of the insensible parts of bodies'.[12] The word 'qualities' he continued to use in its ordinary and popular sense, but he wanted clearly understood that these so-called qualities were derivative from the 'catholick affections' of matter, and reducible thereto. The 'qualities' were secondary, the 'catholick affections' were primary.

And what of God, or the human soul, or the mind and reason? Boyle was a highly religious man. He acknowledged the supremacy of God who had his own ends in view and ordered all activities to bring

these about. But Boyle was perhaps a little indefinite on the question, precisely how rigid was this order? And the difficulty arose because the universe contains more than purely corporeal objects. It contains man, who was provided with a soul, reason, and free will. How firm, then, were the laws governing motion? Boyle declared, in regard to the purely corporeal part of the universe, 'the wise Author of it does seldom manifestly procure a recession from the settled course of the universe, and especially from the most catholick laws of motion'.[13] But God may sometimes 'in a peculiar though hidden way, interpose', but only rarely. And in explaining phenomena we should not hastily 'have recourse to an extraordinary providence' to explain what 'may be probably be accounted for by mechanical laws'.[14]

But while Boyle felt on reasonably firm ground when explaining material objects by corpuscular philosophy, he had much less assurance when he analysed living objects. In his discussion of corpuscular philosophy, he specifically wanted to 'except the reasonable soul that is said to inform the human body'.[15] He could, he felt, readily discuss the 'corporeal works of God', but 'the rational soul or mind of man', which is an immaterial spirit, is quite different. It is 'a substance of so heteroclite a kind' that it could not be readily discussed along with material substance. The body of man and that part which concerns the 'sensitive soul' are material and come under the analysis of matter and motion, but the mind and the rational soul of man do not—or, at least, not with any sort of rigour.

This accorded thoroughly with the Cartesian viewpoint, which clearly distinguished the corporeal part of man from the spiritual and mental part. The Cartesian dualism permitted a good working compromise between science and theology. All animals, and the corporeal part of man, were activated mechanically and lay in the province of science. The immaterial soul of man was not corporeal and not mechanically activated and thus the investigations of science excluded the soul by definition. If we assign the mind to the immaterial realm to which soul belonged, we can avoid much difficulty. But Descartes, and Boyle too, avoided one set of difficulties only by opening the door to other difficulties which, not clearly foreseen at the time, began inexorably to infiltrate and plague both science and philosophy.

The place of mind in any 'mechanical philosophy' has always caused difficulty, and the problem became especially acute in medicine.

Boyle supported the contention that the body of man was a machine, and that physiological activities were, in fact, purely mechanical. In his essay on Nature he attacked the concept that nature—in all its various meanings—was in any way a separate existent, that it could, by any sort of personalized activity, effect the various processes that characterize bodily physiology. The bodily reactions to disease, the crises and the restoration to health following acute illness, or the maintenance of an equilibrium in the face of various disturbances—the 'wisdom of the body' that a later era would call 'homeostasis'—all these phenomena could be explained by purely mechanical factors and did not require any immaterial entity to bring them about. There was no intelligent principle of self-preservation,[16] no intelligent principle *called* nature, no entity which, by exerting intelligence and will, restored health. Instead, the phenomena, asserted Boyle, arose through mechanical means.

As evidence he gave analogies where disturbed equilibrium became restored in a purely 'mechanical' fashion. For example, a metal spring, when distorted from its normal shape, will 'spontaneously return to its former position: and yet here is no internal watchful principle, that is solicitous to make this restitution'.[17] Boyle discussed numerous physiological processes and indicated that they *can* be explained by the configuration and motion of the particles and the interaction of parts. Repeatedly he asserted the body to be a system of interacting parts in motion. The physician should look on the patient's body as an engine that is out of order but yet, 'by his concurrence with the endeavours, or rather tendencies, of the parts of the automaton itself, it may be brought to a better state'.[18] The machine had definite tendencies, all referable to the initial pattern of motion that the Creator had impressed on it. The physician must take account of this trend, sometimes co-operating with it, sometimes contravening it, in order to bring the patient back to health.

In his essay on Nature, Boyle nevertheless manifested considerable vacillation, if not confusion. By indirection he showed the difficulties in reconciling mechanical philosophy with the data of medicine and at the same time with theological doctrines. He tried to straddle the fence. Believing in both God and mechanics, he tried to keep the two sharply separate. God created matter and imparted motion of widely different kinds and potentialities. To preserve his philosophy Boyle could not permit any sort of personalized Nature, any Real

Being of spiritual and intelligent character, to intervene between the creativity of God and the phenomena of the everyday world.

The concept of an active nature, as something real, is familiar in the various expressions as 'Nature abhors a vacuum'; or 'the healing power of Nature'.[19] But in physiological processes it was not 'Nature' as a personalized entity that brought about a crisis or restored the patient's health. The crisis occurred and the patient was restored, but this took place by mechanical processes only, without the intervention of any conscious real agent.

For Boyle 'Nature' was simply a term that designated phenomena but in no way explained them. Boyle was a strict nominalist, and 'Nature' was *flatus vocis*, not a real entity. In regard to nature, he declared, 'The notion I would offer should be some such as this; that nature is the aggregate of the bodies, that make up the world, framed as it is, considered as a principle, by virtue whereof they act and suffer, according to the laws of motion prescribed by the Author of things'.[20] The word is a useful label, but does not represent a Real Being.

To understand the point of Boyle's argument we must appreciate the neoplatonic viewpoint implicit in much medical thinking, particularly in the Paracelsian and Helmontian concepts already discussed. Central to this attitude is a continuity between God, the ineffable One, and the world as we have it. The neoplatonic ideas—emanations or outflow of God into Mind, Soul, individual souls, soul of the world or demiurge; the gradual investment of spiritual substance with finely material nature; and progressive increase in materiality—all this was foreign to Boyle. A continuity between God and the world, central to the Great Chain of Being, introduced factors quite inconsistent with mechanics. And in the 'hydraulico-pneumatical engines we call human bodies . . . things are generally performed according to mechanical laws and courses'.[21]

Having denied any *entity* called nature, Boyle supported his denial by indicating how the mechanical explanations were more elegant and logically satisfying. And he supported his claim by analogies with mechanisms of various types. However, the ultimate weakness of his claims stands out in his rather lame admission that the doctrine depends on faith. Said Boyle, the examples drawn from automata may indeed not be adequate. There was obviously a great difference between an automaton and a man. But 'it is not rational to expect, that in the incomparably inferior productions of human skill, there should be

found engines fit to be compared with these, which in their protoplasts had God for their author'.[22] In other words, the machines that God made are vastly more complex than the machines that puny man can make, for God is infinitely more capable than man. How, then, do we know that God's creations *are* machines? Obviously, because we have the faith that this is the case, even in the absence of actual demonstration. Only faith can bestow conviction on arguments which involve the Deity.

Boyle realized that he could not explain mind in mechanical terms, and yet mind acted on the body. 'Though the body of a man be indeed an engine, yet there is united to it an intelligent being (the rational soul or mind) which is capable . . . to do many of those things it judges most conducive to the welfare and safety of the body it is joined with. So that a man is not like a watch or an empty boat, where there is nothing but what is purely mechanical; but like a manned boat, where, besides the mechanical part . . . there is an intelligent being, that takes care of it, and steers it, or otherwise guides it, and when need requires, trims it . . .'[23] Again, in human bodies which are 'hydraulico-pneumatical engines' things are 'generally performed according to mechanical laws and course', with the proviso, 'when neither particular providence, nor the rational soul, nor overruling impediments interpose'.[24]

However, we must not think that the rational soul, which can intervene in the affairs of the body, is equivalent to the Helmontian Archeus. The rational soul can change the course of the clockwork mechanism or the hydraulico-pneumatical processes. The intelligence determines what changes must take place in order to bring about a restoration to health, but the changes themselves take place mechanically. The Archeus of van Helmont exerted a regulatory function. But for van Helmont the Archeus was *constantly* exerting intelligence in order to bring about bodily functions. For Boyle, on the other hand, most bodily activities took place by purely mechanical means, without the intervention of any rational soul. This reflects the difference between atomism and neoplatonism.

Boyle stressed all those functions which we today call autonomic. Thus, 'in sleep, the circulation of the blood, the regular beating of the heart, digestion, nutrition, respiration, etc. are performed without

the immediate agency, or so much as the actual knowledge, of the mind: and, when a man is awake, many things are done in his body, not only without the direction, but against the bent of his mind . . .'[25] Boyle used the term 'reason' in the sense of *conscious and deliberate direction;* while van Helmont used the term Archeus as a generic activity, the aggregate of forces which preserved the integrity of the body.

Where Boyle spoke about the rational soul directing or controlling the mechanical activities of the body, he was speaking explicitly of the human body. Only the human has soul and reason. Animals do not. Even those animals which, like apes, have bodies similar to man, do not have any incorporeal soul nor a 'rational substance'.[26] For Boyle, as for Descartes, animals were purely mechanical in their physiology.

III

A vast gap exists between the philosophical speculations on ultimate constituents of matter, and the work-a-day world of the practising physician. Yet somehow this gap must be bridged. The efforts to do so come in part from the pure scientist, in part from the actual practitioner; each, starting from his own particular sphere of activity, strives toward the other. Boyle, as a scientist, tried to do his share in bridging this gap, especially in his extremely important work, *The Usefulness of Natural Philosophy*, published in 1663. The first part of this work, on the usefulness of natural philosophy in reference to the mind of man, was written quite early. Boas[27] ascribes its composition to the period 1647 to 1648, Fulton[28] to about 1650. This earlier portion, having relatively little to do with medicine, defended the new philosophy. It indicated that a knowledge of nature will not only give power over nature but will also promote the glory of God and of true religion, and work powerfully against atheism.

The study of nature, starting with concrete observation, seeks an ascending series of causes. For Boyle, as we have seen, the ultimate causal principles (short of Deity) are the 'catholick' properties of matter —shape, size, motion, and 'texture'. If we study the immediate causes of phenomena, 'and desist not from ascending in the scale of causes, till we are arrived at the top of it', we reach these 'primary affections', together with the 'fixt laws of nature (or rules of action and passion

among the parcels of the universal matter)'. And from these we proceed to the First Cause, or God, for we cannot give a 'satisfactory account' of the first principles 'without acknowledging an intelligent author or disposer of things'.[29]

This first part, trying to allay the fears of atheism, was written before Boyle had much first-hand acquaintance with medicine or its problems, or had had significant experience in chemistry. The work is essentially on a verbal plane, and it does not reflect the authority that comes only when an author has concrete experience with the matters he writes about.

The second part of the work, however, which deals extensively with important medical problems, was probably written only a short time before the publication in 1663. In the interval Boyle had acquired a significant knowledge of medicine, had a surprising amount of actual clinical experience, had engaged in much experimental research dealing directly with medical problems (especially those of therapeutics), had come into broad personal contact with leaders in the field, and could speak with considerable assurance. In addition, he had made considerable progress in his chemical investigations and in his elaboration of explanatory theory.

Most 17th century philosophers deeply concerned with science insisted on sharply separating the First Cause from second causes. The First Cause remained the province of religion and theology, the second causes formed the sphere of science. The scientist had a two-fold burden—he must scotch any implication of atheism and then must justify the practical value of the new science in the everyday world. The new science above all wanted to avoid offending the Religious Establishment. And, in addition, the new science—of which the Royal Society was the embodiment in Great Britain—had to justify itself as a serious activity that truly involved the welfare of mankind and did not merely represent the amusements of dilettantes.

Boyle, in Part II of *The Usefulness of Natural Philosophy*, devoted himself largely to the practical advantages of the new science as it affected medicine, but he also expounded the corpuscular philosophy. Man-made mechanisms might be extremely complicated, but the mechanisms of nature were so much more complicated. These latter, so carefully contrived for definite goals, demonstrated for him the foresight of God and enhanced the glory of God. With this connection between God and the world firmly established, Boyle devoted himself

principally to the clock-work mechanisms, and had much less to say about the deity.

His analyses and expositions throw a great deal of light not only on medicine but also on scientific method and the 'logic' of science. Particularly important for our purposes are the processes of reasoning that he used in reference to medicine, the concepts of inference, validity, evidence, and kindred subjects.

Boyle provided important arguments on several different topics. He was a 'naturalist', who believed in studying nature in a more or less systematic fashion. The knowledge of nature thus gained was good in its own right, but in addition it had practical usefulness. However, unlike some of his contemporaries, Boyle had a firm grasp on reality. He knew that various recent discoveries, as, for example, those in anatomy, did not by themselves confer any practical advantage in the practice of medicine. In contrast to so much of the 'modernist' claims in the battle between the Ancients and the Moderns (see next chapter), Boyle's comments were modest and refreshing. 'We observe not, that the late anatomical discoveries of the motion of the chyle and lympha-tick liquor, by formerly unknown ways, in newly detected vessels, hath yet made men cure diseases much better than before.'[30] The new anatomical discoveries—abundant in the middle and latter 17th century —were indeed interesting; they advanced our knowledge of nature; but Boyle did not believe that they were useful in the cure of disease.

However, coupled to his realistic viewpoint was the faith that anatomical studies could eventually have great practical advantages. For example, animal experiments and experimental surgery, as in the ablation of various organs, can 'inform physicians of the true use of the parts of an human body, and help to decide divers anatomical controversies'.[31] Experiments in surgery would thus be not only informative, but would also have heuristic value and significance in proving or disproving contentions.

Implicit, of course, is the assumption that information gained from animals does apply to humans. Boyle indicated that 'a human body it self seems to be but an engine, wherein almost, if not more than almost, all the actions common to men with other animals are performed mechanically'.[32] In his somewhat obscure English Boyle is indicating the kinship of man and animals, at least so far as their bodies are concerned. The human mind, and its connexion with the human soul, were not in issue. Descartes had effectively set the human soul

apart from the hydraulic and other mechanisms operative in the human and animal bodies. Boyle was following this tradition.

The kinship of all nature was a basic postulate for the new science. This granted, the activities of the naturalist take on new practical importance for the welfare of mankind. For example, the naturalist can investigate diseases of animals which often resembled those of humans. And experiments can be performed in animals that cannot be carried out on humans. Such experiments may 'confirm or confute' one or another theory and put an end to controversies, and help to find the true causes of disputed phenomena.[33]

The study of 'anatomy', which included physiology and experimental surgery, *could* have profound effects on medicine. At the time that Boyle was actually writing, these effects were perhaps more suggestive than actual. Boyle realized this, but he had great faith in the future benefits. He relied on faith and enthusiasm, and tried to convey his message by these means rather than by step-wise demonstration of concrete benefits.

If the naturalist wanted to make science serve medicine, he should above all study chemistry. This could throw much light on physiological processes as well as on pathology—for example, the process of digestion or the nature of kidney and bladder stones, or the activity of therapeutic remedies. Boyle, however, had no illusions on the positive achievements that chemistry had as yet wrought on medicine. In regard to everyday practical benefits, there was much to be desired. Said Boyle, 'For our vulgar chymistry . . . is as yet very incomplete, affording us rather a collection of loose and scattered . . . experiments, than an art duly superstructed upon principles and notions, emergent from severe and competent inductions.'[34] He realized that the 'pure' science of chemistry needed to make vastly more progress before practical results could occur. 'And therefore, till the principles of chymistry be better known, and more solidly established, we must expect . . . that very few vulgar chymical remedies should be of the noblest sort; and that . . . considerable errors should be wont to pass unheeded, and faults . . . mistakenly committed.'[35]

Boyle had the scientific ideal, that a science should rest on certain general principles, arrived at through careful reasoning from observations. These principles could then give rise to an 'art' or practical applications—applied science—whose usefulness rested upon the validity of the basic concepts. But actually, in the 17th century, chemistry was rather a scrap-heap of miscellaneous and unsystematized experiments

which could not yet provide the 'severe and competent induction' needed for proper science. Until the basic principles were better established, the application of chemistry to medicine would involve considerable error. Nevertheless, chemistry need not wait for a complete set of principles, for if men have 'curiosity and industry to vary and prosecute experiments', they will most certainly discover new and valuable remedies.[36]

Even in the imperfect state of chemistry, whoever knew this subject would have a much better basis for discussing the 'changes and operations of the humours, and other juices contained in the body'.[37] He would examine the properties of urine, blood, digestive fluids, lymph, and the other humours, determine their various qualities and reactions with other substances. Boyle indicated that chemical processes were the same in both macrocosm and microcosm, in the great world of nature and in the body of man. The chemist who had studied chemical processes in general was more 'likely to explicate divers particulars in pathology more intelligibly, than he that is a stranger to chymistry'.[38] If we substitute 'physician' for 'chemist', we have a remarkable forecast of the 20th century.

In hygiene and therapeutics, chemistry might have profound influence. Boyle, relying on faith rather than on concrete achievement, had almost a prescience in indicating what might be accomplished. Chemical studies on food and drink, on the nature of fermentations, on modes of preservation, could improve the health of mankind. But he indicated that he was talking more about the future than the past, about possibilities rather than actualities. He emphasized that, while observations could bring a lot of new information, they should be carried out systematically and with sound experimental method, before general rules could be framed.

Chemistry, clearly, had much to offer in the study of therapeutic agents and here, especially, it could greatly advance medical knowledge. The chemist could seek the active principles of drugs, their modes of action, try to find new and better ways of preparation, or discover quite new and hitherto unsuspected remedies. Boyle's account of what *might* be accomplished is indeed impressive, when read three hundred or more years later. But in his own day it represented largely an expression of faith. Boyle had great religious faith,[39] but he also had great scientific faith.

IV

Boyle was an 'empiricist'. This word, which trips so easily off the tongue and constitutes such a useful label, is not easy to define. The empiricist relies on 'experience', rather than on 'reason', but obviously these terms are relative. We cannot have reasoning without some experience to work on; and we cannot render experience orderly without reason. Instead of relying on labels, we should study Boyle's actual procedures.

Boyle wanted to collect *facts*. He had a greater regard for facts than for opinions, and he indicated that an author who gives merely his reasonings can be in error and carry us into the same error. But if his writings were 'competently stocked with experiments, it is the reader's own fault, if he be not a learner by them'. The writer who gives new observations to support his opinions may have erroneous opinions but the experiments can carry weight. Even if faulty reasonings have been erected upon the experiments, yet if the foundation is solid, 'a more wary builder may be very much furthered by it in the erection of more judicious and consistent fabrics'.[40]

Opinion is fallible, inference may be specious, reasoning can land us in error. *Fact*, however, has stability. Yet any such distinctions entail the significant problem, What *is* a fact, and how do we determine it? How to characterize an event as fact? Boyle did not face this problem squarely but he did at least touch on it by indirection, without providing a definite solution.

When we study Boyle's writings we find that he often distinguished the reports of others from what he observed with his own eyes. Indeed, Boyle made many pronounced distinctions between what he himself had seen and what other people reported to have seen. Thus, he mentioned 'a very experienced and sober gentleman, who is much talked of' who cured cancer of the female breast 'by the outward application of an indolent powder, some of which he also gave me'. But, he added cautiously, he had not yet 'had the opportunity to make trial of it'.[41] Since he himself could not test the alleged remedy, he withheld judgment. Or, again, he spoke of 'sober travellers' who made claims regarding the treatment of poisons. But, he said, 'having not yet made any trial of this my self, I dare not build upon it'.[42]

There are many such instances scattered through his works, where he reported an alleged cure but specifically indicated his own mental reservations. He was cautious in accepting the statements of others, even of those who were acknowledged to be 'sober', 'experienced', or even 'judicious'.

His scepticism extended to well-known authors, such as Paracelsus and van Helmont. Their specific remedy against 'the stone', for example, and the claims they made that they could reduce stones to 'insipid water, is so strange (not to say incredible) that their followers must pardon me, if I be not forward to believe such unlikely things, til sufficient experience hath convinced me of their truth'.[43] He remained sceptical of the powder of sympathy, although he did not definitively exclude it from credence.[44] And similarly with so-called 'transplantation' where, for example, therapeutic substances applied to an affected part would be removed and placed into a hole bored into an oak root, following which the pain in the patient would cease. But if the material were removed from the hole the pain would recur. Boyle gave examples drawn from respected authorities, but he felt that these narrations were perhaps suspect.[45] And while he remained sceptical of 'magnetical ways of cure', he thought it only appropriate that 'further trial be made of physical experiments of this kind'. And he went on to say, 'if they really do sometimes succeed, though sometimes they chance to fail, yet that possibility of their succeeding may sufficiently evince, that there are really in nature medicines, that work after that extraordinary manner'.[46]

But while he remained cautious in accepting assertions of cure or assertions of causal relationship, he was equally insistent that investigators should keep an open mind. He was constantly pleading for physicians to enlarge their experience, to try new medicines, even though these were not based on traditional doctrine. We should all learn from others, even from those who are not trained physicians. Thus, he felt that physicians could profitably 'collect and digest all the approved experiments and practices of the farriers, graziers, butchers, and the like, which the Ancients did not despise'.[47] All claims of cures were worthy of at least respectful attention, and snobbishness in collecting medical data should be avoided.

Boyle, although not a physician, had a fairly considerable medical experience. He had considerable scepticism regarding some therapeutic claims and alleged successes that other people had reported. But where

he himself was involved, where he had provided the remedy and followed the case, he was much less critical.

He relied quite substantially on casual experiences with patients. Thus, in regard to the effectiveness of millepedes in disease, he declared he believed in their 'efficacy', since he had tried them on himself and found them to be 'very diuretical and aperitive'. And then he considered millepedes very effective in suffusions of the eyes[48] on the evidence of a single patient who had lived in the same house with him. Again, he knew of an 'ancient gentlewoman' who every morning drank some of her own urine 'by the use of which she strangely recovered'.[49] Where he had some firsthand experience with a medication he was more willing to believe in its curative value than if the report were merely at secondhand. As a confirmed hypochondriac he treated himself often and vigorously; and as a naturalist and virtuoso, he came into abundant contact with sick people, either directly or through the mediation of physicians. He was thoroughly willing to accept *post hoc propter hoc* reasoning in individual instances.

As a chemist he worked extensively with medicinal substances. His goal in large part was the pursuit of knowledge for its own sake, as befitted a virtuoso, but in addition, as representative of the new science, he held that knowledge should yield 'fruit'—i.e., confer benefits on mankind. And medications could be adjudged beneficial only if they were actually tried out on patients. Thus, Boyle was one of the early clinical pharmacologists as well as a chemist.

Since he had studied intensively the action of drugs and was, in addition, a genial and approachable person, he was liberal with advice, both to patients and to consulting physicians. With the latter he maintained excellent relationships, for he did not in any way compete with them—he said specifically that he 'was careful to decline the occasion of entrenching upon their profession'.[50] He was quite capable of getting physicians to make trials for him, and he indicated that they did so 'very willingly'.

Boyle provided an instructive discussion of therapeutic trials which throws considerable light on his scientific methodology. He had had great interest in a specific remedy that van Helmont had discussed. In one treatise[51] van Helmont described the remarkable cures brought about by a man identified only by his name, Butler, an Irishman who came to world view while in jail. Apart from Helmont's account, he does not appear in medical annals.

A fellow prisoner was a Franciscan monk who had a severe erysipelas of the arm. Butler pitied him and, to cure him, took a very special stone that he had, dipped it briefly in a spoonful of 'almond milk', and bade the jailer convey a small quantity of this into the food of the monk. Almost at once the monk, not aware of the medicine, noted an extremely rapid improvement. Van Helmont related many other cures brought about by the stone. The mode of action he considered to be that of a ferment, where an extremely minute quantity of agent could induce a tremendous effect. Like a ferment, the power of the stone could become manifest even in enormous dilution and it could impart its remedial virtue to a vast quantity of solvent. Moreover, the stone seemed to be effective against a wide variety of diseases, with a sort of universal power.

Van Helmont, by chemical experiment, had tried to reproduce a medicine which would have the curative virtue of Butler's stone. Boyle was quite impressed by this and he tried to improve on Helmont's technique. He described, with explicit directions, his own mode of preparation, unnecessary to repeat here.[52] The resulting substance represented a purified medicine which he prescribed in variable dosages. It was, he declared, a 'potent specifick for the rickets', since he, and others to whom he had given it for use, had 'cured' a hundred or more children of that disease. In addition, he prescribed the medicine in fevers and headaches, and he thought it 'hath done wonders' in obstinate suppression of the menses. It was also quite successful in improving the appetite.[53]

Unfortunately we do not know what ailed the patients who received Boyle's preparation. We do not know the symptoms, course, or duration, nor the criteria for diagnosis and for cure. We can assume, perhaps, that Boyle was well read in the medical literature of the day, for many well educated men were widely read in the theory and practice of physick. But probably he used this medicine in a 'shot-gun' fashion and gave it quite uncritically in a wide variety of diseases. Perhaps the large number of children receiving the medicine indicated that he chose his patients wherever he could get them. However, for our purposes the important feature concerns the assertion of cure, and the belief that the medicine was effective.

In another example, he indicated his own way of making essence of hartshorn. This he prescribed in small doses and found that it 'resists malignity, putrefaction, and acid humours'. He used it 'in fevers,

coughs, pleurisies, obstructions of the spleen, liver, or womb, and principally in affections of the brain'.[54] There is no hesitation here, no scepticism. He prescribed the medicine and he saw improvement and he had no doubts concerning the cause-and-effect relationship, nor the validity of his alleged facts.

By present-day standards he lacked any adequate criterion for either diagnosis or cure. Various patients were ill of various diseases; he gave them one or another preparation; the patients recovered; the medicine was the effective agent. This chain of reasoning has always attended medical practice. But in contrast to the average practitioner the scientist examines evidence critically and questions its validity. In chemistry Boyle was a first-class scientist by any standards, but as a clinician he was quite uncritical. In some instances, as we have seen, he questioned the evidence that others had offered, but where his own observations were concerned, he did not question.

We get a rather different view of his critical powers if we examine his views on so-called specific medicines. This term is intimately connected with the dominant Galenic doctrines. A 'specifick' represented a curative agent whose mode of action could not be explained by reference to the Galenic qualities or manifest properties. That is, the mode of cure could not be attributed to the properties of hot or cold, moist or dry, or the composite qualities; nor to manifest action on the humours or the temperaments. An example would be the cure of haemorrhage by the wearing of a jasper. The belief in jasper as an anti-haemorrhagic agent represents part of the 'doctrine of signatures'.[55] A red stone was considered good for diseases of the blood, comparable to the way that saffron was good for jaundice. Boyle related, from the literature, a case of a girl subject to 'vehement' haemorrhage for six years, in whom all remedies failed. When her physician hung around her neck a piece of jasper, 'of whose virtues in such cases he had made good trial',[56] the bleeding stopped. And by way of control, when the stone was removed, the bleeding began again, but stopped when once more the stone was placed around her neck. The Galenic explanation would be that of an 'occult' quality, a specific virtue, curative power, or healing quality, as a brute and ultimate datum. Since the Galenic qualities represented the Real, an occult or specific quality would have a metaphysical status comparable to manifest qualities. The haemorrhage-stopping property in a particular stone would be comparable to the quality *hardness* in a piece of iron. While we might think of all this

as merely an *ad hoc* attribution, using the *explicandum* as the *explicans*, and only a verbal gymnastic, this is an unfair criticism. So long as the qualities—manifest or occult—represented a primary reality (and not merely a 'derivative' property), the explanation is at least logical, even if not 'correct'.

Boyle gave many other instances of cures brought about by agencies that seemed to work in a 'secret and unknown manner'.[57] Cures attributed to the weapon-salve, and the powder of sympathy, or to magnetic effluvia, would be of this character; or the cures that take place through strong emotion, or the alleged virtues of 'transplantation'. (See pp. 57, 77.) The cure of a scorpion bite by application of scorpions infused in oil[58], or the cure of a tarantula bite by soft music[59], would be of this type. The Galenic philosophy could not explain these data by the routine principles, such as we have already discussed in the first part of chapter two. The only Galenic explanation would be that of an 'occult' and 'specific' quality.

Boyle indicated some interesting features of the contemporary medical outlook. Numerous physicians, he declared, denied the existence of such specifics, and rejected remedies that do not act 'by visibly evaluating peccant humours'.[60] These physicians rejected specifics because 'they cannot clearly conceive the distinct manner of the specificks working' and cannot conceive how a medication may be subjected to bodily 'digestions' and 'be whirled about with the mass of blood to all the parts', and yet be highly selective, to 'shew it self friendly to the brain (for instance) or the kidneys, and fall upon this or that juice or humour rather than any other'.[61] In other words, the inability to provide a suitable *explanation* in terms of basic Galenic principles had led many doctors to deny the *fact*.

Clearly, two separate problems are here involved. First, *do* these medications work? Is it a *fact* that the specific medication cured the specific disease? Only if we assume an affirmative answer do we encounter the second problem. What is the explanation? There are many facts that elude ready explanation. The scientist is the one who seeks the causes, reasons, and explanations, and tries to refer the phenomena to first principles of the medical sciences. The explanation offered may or may not be successful. But Boyle condemned those who denied the fact simply because they could not furnish a suitable explanation. Boyle was thus condemning the rationalistic viewpoint, which determines truth in experience by its consonance with rational requirements. In

this instance, the term 'rational' meant congruence with antecedent theories.

Boyle, on the other hand, had a far greater respect for fact and empirical observation. As we have already indicated earlier in this chapter, Boyle did not want to dismiss medical claims and medical experiences without thoughtful examination. He kept an open mind, was constantly seeking to enlarge his experience, and wanted to draw upon the experience of all who had contact with illness, whether human or animal. 'Collect observations' was his watchword, and while we might regard him as unduly credulous, we must recognize his zeal for amassing data, and his condemnation of *a priori* elimination of possible data. While often cautious in accepting at face value, he would not reject alleged cures outright. He merely desired further study and further data. When we say that he was credulous, we really mean that he did not have the tools for distinguishing the valid claims from the invalid.

He did, however, have certain procedures at his disposal which are highly significant for understanding the 17th-century point of view. He demonstrated that the (alleged) phenomena *could* be explained by the corpuscular philosophy. He was dealing in hypothetics, indicating how specifics *might* work according to the tenets of corpuscular philosophy. This philosophy, he felt, furnished the best explanation for natural phenomena, better than the modified Aristotelianism that buttressed itself on the Substantial Forms, better than the three Spagyric principles of mercury, sulphur, and salt, better than the Helmontian doctrine of the 'middle life'.

In his discussions, he intermingled analogies and examples, to give some concreteness to the more abstract assertions. For example, certain well-known substances, if applied to the skin, could exert a specific action on some internal organ. Cantharides, externally applied, would 'work' upon the bladder; and mercury ointment rubbed on the skin could have as great effect in producing salivation as if the mercury were swallowed. And some cathartics would have a purgative action if merely rubbed on the skin. These data, unquestioned facts of observation, could readily be explained by corpuscular philosophy, with the passage of fine particles through the skin into the blood, and dissemination through the body, with specific action on a given organ.

Given this well-attested evidence with its completely probable explanation, it required no great leap to assume that in various amulets, or

other specifics, some 'invisible corpuscles' may also get into the blood and exert their effect. The force of the argument lies in the analogy. If a given mechanism can explain one phenomenon which no one doubts, a similar mechanism may be at work in some other phenomenon—even though that phenomenon is not universally accepted. It *might* be a fact, and if it is, it *could* be explained in this fashion.

Boyle did get rather hazy at times. He assumed that by the 'swift motion, and by the figure of the corpuscles' [i.e., the pattern of the particles composing the medicinal agent] there could be induced 'such a new and unnatural impediment or determination to the motion of the blood, as to discompose either its texture or that of the heart, brain, liver, spleen, or some such principal part of the body . . . as a little leaven is able, by degrees, to turn the greatest lump of dow [dough] into leaven'.[62] Specific poisons could act in the same fashion and so could specific remedies.

Elsewhere[63] he gave a more detailed and yet rather confused account of the possibilities. The human body is an engine, and there are 'many strainers . . . of differing textures'—by which he means the different organs—and there may be 'divers local ferments' in the particular parts. The blood mass continuously streams through all parts of the body and a medicine could be readily carried from one part to another. A medication in the blood, as it passed through these strainers, could undergo alteration 'whereby the size, or figure, or motion of its small parts may be changed'. Then, 'By that time the medicine (crumbled as it were in minute corpuscles) arrives at the part or humour to be wrought upon, it may have a notable operation there . . . because the motion, size, or shape of the medicinal corpuscles in the blood, though not by sense distinguishable from the rest of the liquor they help to compose', yet may be able 'to restore the tone or texture of the strainers, or alter the blood, or resolve and carry away such tenacious matter, as stuffed or choaked up the slender passages of the strainer, or at least straitned its pores, or vitiated their figure'.

Of course, all of this is sheerest speculation, but it is all at least logically plausible, and accords with the basic principles. Boyle continued the exercise in hypothetics by saying, 'Thus a specifick for one disease may be resolved in the body into minute particles of such figure and motion, that being fit to stick to other corpuscles of peccant matter, which, by their vehement agitation, or other offensive qualities discompose the body and make it feverish, may allay their vehement

motion, and by altering them, as to bigness and shape, give them new and innocent qualities, instead of those noxious ones they had before.'[64] Other specifics can act upon the humours or relieve obstruction and so on. Different specifics would act in different ways. Boyle was thus constructing a hypothetical mechanism which would explain the data of medication and clinical cures. He was 'spelling out', so to speak, a possible mechanical explanation in terms that were consistent and plausible, even if not necessarily cogent.

Boyle was entirely familiar with phenomena whereby invisible particles could affect the human body. Infectious diseases propagated themselves by effluvia that could not be detected. Odours, invisible, could produce profound effects. And even the thoughts and desires of the mother, it was widely believed, could make an impression on the unborn infant. These were all considered part of natural processes. The so-called specifics would fall into the same category. We should not reject the possibility of specifics without trial.

The problem, What constitutes a suitable trial? had no accepted solution. Certain remedies were generally well regarded. A physician should first use the ordinary means of cure, but if these were not effective, he might try unproven remedies. 'If the physician be duly satisfied of the efficacy of the remedy, upon a competent number and variety of patients, I suppose he may, without rashness, make use of such remedies, at least where ordinary medicines have been already fruitlessly tried.'[65] There was no clear indication how a physician could get 'duly satisfied' or what constituted a 'competent' number.

The question of suitable rationale had particular importance. The empiric used remedies without having any sound theoretical basis. He used them just because experience had shown that they work. The scientific physician, however, could give reasons for what he did. Boyle, by his theoretical and hypothetical explanation, tried to confer scientific respectability on medications that might have only the empiric's recommendation—not a high recommendation for a scientist. Boyle emphasized what Celsus had said, that the art of medicine was not discovered by reason, but rather that medicines were first discovered, and then the reasons found out.[66] It was a great service to insist that physicians should keep an open mind, and not try to circumscribe the operations of nature through their own narrow theories, for nature can do things that we do not anticipate or understand. At the same time Boyle realized that unusual allegations need 'good proof to

make a wary man believe'[67] but he did not tell us what constitutes proof.

From the standpoint of scientific method, Boyle clearly showed that the Galenic theory was less capable of explaining strange phenomena than was the corpuscular theory. The latter had greater flexibility in explanation, and therefore a greater potentiality. This, of course, did not make it true but did give it an aesthetic advantage. Keeping the corpuscular philosophy as his first principles, and showing an earnest respect for phenomena and a desire to make fair trials, Boyle greatly advanced the scientific method in medicine.

In this connection Fisher[68] has discussed many aspects of Boyle's methodology. Four obstructions had hindered the development of experimental method:[69] (1) explaining phenomena by reference to 'nature'; (2) explaining physical phenomena by Substantial Forms; (3) deference to authority in the testing of opinions; (4) using abstract reasoning without sufficient observation and experiment. These obstructions Boyle cleared away.

Boyle, continues Fisher, was not devoted particularly to the mathematical method but rather to the observation of phenomena. He was opposed to philosophical systems based upon abstract reason, and considered mathematics accessory and auxiliary to experiment. Mathematical reasoning reached but a very small part of phenomena. Boyle was still in the stage of natural history, of collecting observations, but the observations had to be collected in a systematic, orderly, and experimental fashion, and not merely at random.

Fisher emphasizes that Boyle had a sharp insight into the processes of induction.[70] He indicated three types of experiments—showing positive instances where a quality might be mechanically introduced; showing how the quality might be varied in degree; and then, how the quality could be abolished or eliminated from where it had been present.[71] The experiments thus involved introduction, variation, and expulsion, to indicate validity. Fisher, believing that Boyle's method avoided the extravagance of Bacon and the over-emphasis on mathematics of Descartes,[72] considers Boyle to be the father of experimental method.

Perhaps the term 'father' is too strong, so far as concerns science in its broad sense. But certainly Boyle, without being a physician, gave a tremendous impetus to scientific methodology in medicine. He emphasized the experimental method, the need to try things and the desir-

ability of adopting them if they worked. He appreciated the heuristic value of theory, and realized that elegance and aesthetic criteria should play a part in evaluating competing theories. Many phenomena defy rigorous explanations. These must be accepted as brute fact and yet may be subjected to tentative explanations of speculative character.

The great drawback, perhaps, was the inability to provide satisfactory answers to the question, What *is* a fact? If Boyle could not provide an adequate answer, we can readily understand how physicians with less genius, less scientific aptitude and less critical acumen, could fall into serious error.

Boyle made profound contributions to science, and the specifically medical sciences benefited at least indirectly. Especially significant for us was his concern with the philosophical basis and methodology of science, and this led to important changes in both the theory and practice of medicine. His theoretical concepts helped to undermine the Galenic views and to permit an alternative that could hold the allegiance of progressive physicians. He struggled with the problem of 'qualities' and was able to dispense with the notions of 'forms', 'seeds', and 'occult qualities', even though he had to fall back on other hypothetical terms. When he attended to medical problems, he laid particular emphasis on methodology, on theory, hypothesis, and explanation, and in the present context this constitutes his special importance.

Interestingly enough, although Boyle has achieved a particular reputation as a chemist, so far as medicine is concerned, he had but little effect on the trend to chemical explanation. To study this trend we must turn to the so-called iatrochemists, who carried on the tradition of Paracelsus and van Helmont. I choose, as representative of latter 17th-century iatrochemistry, the justly renowned physician Franciscus Sylvius.

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Van Helmont

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17. For 16th and 17th century neoplatonism see particularly Pagel (reference 2, 1962), as well as Ernst Cassirer, *The Platonic Renaissance in England*, translated by J. P. Pettegrove (Austin: The University of Texas Press, 1953); John Tulloch, *Rational Theology and Christian Philosophy in England in the Seventeenth Century*, 2 vols., Vol. II, *The Cambridge Platonists* (Edinburgh and London: William Beachwood and Sons, 1872); Frederick J. Powicke, *The Cambridge Platonists* (Cambridge, Mass.: Harvard University Press, 1926).
18. For the study of Plotinus the indispensable documents are *Plotinus, The Enneads*, translated by Stephen MacKenna, 2nd ed. (London: Faber and Faber, ND); and W. R. Inge, *The Philosophy of Plotinus*, 2nd ed. (London: Longmans, Green and Co., 1923).
19. 'The Chief or Master Workman [*Archeus Faber*],' §2.
20. *Ibid.*, §3. The translation is mine, not Chandler's.
21. *Ibid.*, §§3, 4.
22. 'The Image of the Ferment,' §29.
23. *Ibid.*, §§8–10.
24. *Ibid.*, §8. Chandler has given a baffling translation of this passage, as '. . . the seminall *Archeus* which aspireth by its flowing into a living soul'.
25. *Ibid.*, §12.
26. *Ibid.*, §8.
27. *Ibid.*, §§18–20.
28. *Ibid.*, §17.
29. *Ibid.*, §23.
30. Robert P. Multhauf, 'J. B. van Helmont's Reformation of the Galenic Doctrine of Digestion,' *Bull. Hist. Med.* 29: 154–163, 1955; Walter Pagel, 'J. B. van Helmont's Reformation of the Galenic Doctrine of Digestion—and Paracelsus,' *Ibid.*, *29*: 563–568, 1955; 'van Helmont's Ideas on Gastric Digestion and the Gastric Acid', *Ibid. 30*: 524–536.
31. The principal texts are 'Heat Doth not Digest Efficiently' and 'A Sixfold Digestion of Human Nourishment'; and to a lesser extent, 'The Threefold Digestion of the Schools'.
32. 'Heat Doth not Digest,' §11.
33. *Ibid.*, §12.
34. *Ibid.*, §23.
35. 'Sixfold Digestion,' §14.
36. 'Heat Doth not Digest,' §28. The translation is my own, not Chandler's.
37. *Op. cit.*
38. 'Sixfold Digestion,' §20.
39. Lester S. King, *The Growth of Medical Thought* (Chicago: University of Chicago Press, 1963), p. 182.
40. 'Sixfold Digestion,' §29.
41. *Ibid.*, §61.

42. 'The Blas of Man,' §§21, 22.
43. *Ibid.*, §22; 'Sixfold Digestion,' §62.
44. 'The Blas of Man,' §45.
45. Lester S. King, *op. cit.*, pp. 126–129.
46. 'Magnum Oportet,' §48.
47. *Ibid.*, §25.
48. *Ibid.*, §28.
49. *Ibid.*, §29.
50. *Ibid.*, §52. The translation is mine, not Chandler's.
51. *Ibid.*, §54.
52. *Ibid.*, §57.
53. In this connection, see 'Of Archeal Diseases,' *passim*.
54. *Ibid.*, §§15, 16.
55. 'Magnum Oportet,' §§52–59.
56. 'Birth or Original of a Disease Image' ['Ortus Imaginis Morbosae'] §6, translation mine.
57. *Ibid.*, §13.
58. Walter Pagel, *Joh. Baptista van Helmont: Einführung in die philospohische Medizin des Barock* (Berlin: Springer, 1930).
59. On van Helmont, J. R. Partington, *A History of Chemistry*, vol. II (London: Macmillan and Co., 1961), pp. 209–241, [with excellent bibliographies]; see also, R. P Multhauf, *The Origins of Chemistry* (London: Macdonald, 1966), especially Chapters X and XI.

Boyle

1. The writings of Marie Boas Hall present the best over-all view of Boyle as a chemist, and as a member of the 17th century scientific community. Particular reference may be made to *Robert Boyle and Seventeenth Century Chemistry* (Cambridge: The University Press, 1958) and to the introductory chapters of *Robert Boyle on Natural Philosophy* (Bloomington, Ind.; Indiana University Press, 1965).
2. An excellent but little known study is Mitchell Salem Fisher, *Robert Boyle, Devout Naturalist: A Study in Science and Religion in the Seventeenth Century* (Philadelphia: Oshiver Studio Press, 1945).
3. Robert Hugh Kargon, *Atomism in England from Hariot to Newton* (Oxford: Clarendon Press, 1966), contains a superb bibliography, with critical annotations. In addition to Kargon's book I have derived particular benefit from Marie Boas, 'The Establishment of the Mechanical Philosophy', *Osiris* (1952), *10*: 412–541; F. A. Lange, *The History of Materialism* (third ed., translated by E. C. Thomas; London: Routledge and Kegan Paul, Ltd., Reprinted 1950, from 1892 ed.); Cyril Bailey, *The Greek Atomists and Epicurus* (Oxford: Clarendon Press, 1928); Kurt Lasswitz, *Geschichte der Atomistik vom Mittelalter bis Newton* (2 vols.; Hildesheim: Georg Olms, 1963 [reprint of original 1890 ed.]); Lancelot Law

Whyte, *Essays on Atomism: From Democritus to 1960* (Middletown, Conn.: Wesleyan University Press, 1961).

4. Walter Charleton, *Physiologia Epicuro-Gassendo-Charltoniana: Or, a Fabrick of Science Natural, upon the Hypothesis of Atoms* (London: T. Heath, 1654 [Facsimile reprint, with introduction by Robert Hugh Kargon (New York: Johnson Reprint Corp., 1966)]).
5. All citations from Boyle come from *The Works of the Honorable Robert Boyle*, 6 vols., ed. Thomas Birch (London: W. Johnson et al., 1772 [Reprinted Hildesheim: Georg Olms, 1966]). References will give the title of the particular text, and the volume and page of this edition, hereafter identified as *Works*.
6. 'A Free Inquiry into the Vulgarly Received Notion of Nature' [hereafter identified as 'Nature'], *Works* V. 179.
7. 'The Origin of Forms and Qualities According to the Corpuscular Philosophy,' *Works* III. 15.
8. *Ibid.*, p. 16.
9. *Ibid.*, p. 22.
10. *Ibid.*, p. 23.
11. *Ibid.*, p. 24.
12. *Ibid.*, p. 26.
13. 'Nature,' *Works* V. 215.
14. *Ibid.*, p. 216.
15. 'Origin of Forms,' *Works* III. 12.
16. 'Nature,' *Works* V. 211.
17. *Ibid.*, p. 233.
18. *Ibid.*, p. 236.
19. Max Neuburger, *Die Lehre von der Heilkraft der Natur im Wandel der Zeiten* (Stuttgart: Ferdinand Enke, 1926). A rather unsatisfactory translation has been made by Linn J. Boyd (with no bibliographic data of publisher or date).
20. Boyle, 'Nature', *Works* V. 177.
21. *Ibid.*, pp. 238–9.
22. *Ibid.*, p. 236.
23. *Loc. cit.*
24. *Ibid.*, p. 238.
25. *Ibid.*, p. 241.
26. *Loc. cit.*
27. Marie Boas Hall, *Robert Boyle on Natural Philosophy* (Bloomington, Ind.: Indiana University Press, 1965), p. 16.
28. John F. Fulton, *A Bibliography of the Honourable Robert Boyle* (Second ed.; Oxford: The Clarendon Press, 1961), p. 37.
29. 'Of the Usefulness of Natural Philosophy,' *Works* II. 37.
30. *Ibid.*, p. 163.
31. *Ibid.*, p. 68.
32. *Ibid.*, p. 76.
33. *Ibid.*, p. 85.
34. *Ibid.*, p. 145.
35. *Ibid.*, pp. 145–146.

36. *Ibid.*, p. 149.
37. *Ibid.*, p. 79.
38. *Ibid.*, p. 80.
39. Richard S. Westfall, *Science and Religion in Seventeenth Century England* (New Haven: Yale University Press, 1958), pp. 40–41, 124–127 and *passim.*
40. 'A Proëmial Essay,' *Works* I. 303–304.
41. 'Usefulness,' *Works* II. 74–75; 116.
42. *Ibid.*, p. 87.
43. *Ibid.*, p. 97.
44. *Ibid.*, p. 165.
45. *Ibid.*, pp. 165–166.
46. *Ibid.*, p. 167.
47. *Ibid.*, p. 169.
48. *Ibid.*, p. 131.
49. *Ibid.*, p. 130.
50. 'Memoirs for the Natural History of Human Blood,' *Works* IV. 637.
51. 'Butler.' This appears in the *Oriatrike*, pp. 585–596, and in the *Ortus Medicinae*, pp. 358–364. See references 4 and 5, p.88.
52. 'Usefulness,' *Works* II. 135–136.
53. *Ibid.*, p. 136.
54. *Ibid.*, p. 138.
55. Lester S. King, 'The Road to Scientific Therapy: "Signatures," "Sympathy," and Controlled Experiment,' *J. Am. Med. Assn* (July 25, 1966), *197*: 250–256.
56. 'Usefulness,' *Works* II. 160.
57. *Ibid.*, p. 170.
58. *Ibid.*, p. 190.
59. *Ibid.*, p. 181.
60. *Ibid.*, pp. 170, 175.
61. *Ibid.*, p. 190.
62. *Ibid.*, p. 183.
63. *Ibid.*, pp. 191–192.
64. *Ibid.*, p. 192.
65. *Ibid.*, p. 185.
66. *Ibid.*, p. 193.
67. *Ibid.*, p. 195.
68. Fisher, *op. cit.*, pp. 26–64.
69. *Ibid.*, p. 36.
70. *Ibid.*, p. 51.
71. In this connection see Boyle, 'Experiments, Notes, etc., about the mechanical Origin or Production of divers particular Qualities', *Works* IV. 232.
72. Fisher, *op. cit.*, pp. 62–64.

Chapter Three

Methodology

SYLVIUS

RIVERIUS, Helmont, and Boyle illustrate three different types of philosophy. Their teachings were permeated with metaphysics and we cannot appreciate either the doctrines themselves or their subsequent effects without appreciating the philosophical substrates. Sylvius, on the other hand, did not represent any metaphysical school. He expressly rejected the philosophical subtleties of Helmontian medicine[1] and turned his back on its neoplatonic overtones. He had little concern with the new atomism of Gassendi or Descartes; and although he maintained some of the Galenic concepts, he did not actively support the Galenic philosophy and the Galenic explanations as we have seen them expounded in Riverius.

Franciscus de le Boe, called Sylvius, descended from a well-to-do Protestant family which came from Cambrai and whose original name was Dubois. For religious reasons they had migrated to Holland, but because of disturbances there withdrew later to Hanau, where Franciscus was born in 1614. He received his education in Sedan and in Dutch and German universities, and finally got his doctor's degree in Basel, in 1637. Further periods of study carried him to Paris and to Leiden. In 1641 he took up practice in Amsterdam, where he made such a resounding success that in 1658 he was called to the University of Leiden as professor of medicine. Here he remained for fourteen years until he died in 1672.

Although he won great renown as a practitioner and as a teacher, he was not an experimentalist and did not carry on significant original researches. Yet, his broad education and extensive exposure to current

thinking readily fitted him to adopt new attitudes in science, especially those that derived from the chemical teachings of van Helmont. These concepts, however, came to him at second hand, i.e., from the works of others rather than from his own original researches. He was a leading member of the so-called iatrochemical school that tried to explain physiological and pathological phenomena by chemical concepts. Paracelsus and van Helmont were the earlier exponents. Sylvius and Thomas Willis carried on the tradition in the 17th century. Interestingly enough, later physicians of prominence who were also eminent chemists, such as Stahl and Boerhaave, quite rejected chemical explanations for biological phenomena, and are not included in the category of iatrochemists.

Sylvius we must regard not as a metaphysical thinker but rather as an early and rather feeble positivist. An essentially practical sort of person, he made hypotheses which would explain the facts of experience, but he did not enter into any higher reaches of abstraction. Instead, he tried to explain biological phenomena in terms relatively close to experience and theoretically amenable to direct observation. He tried to explain medical phenomena by the chemistry of the day, without recourse to metaphysics.

Unfortunately, Sylvius has not received much detailed attention in the English historical literature. He wrote in Latin and little has been translated. One major work, the first book of his *Praxeos Medicae Idea Nova*, was translated into English in 1674.[2] Not only is it rare, but even more important, the English is far from lucid to present-day readers. More recently, Foster[3] provided a readable translation of small excerpts, but never gave textual references to what he translated.

Even good secondary source material is rare in English. The best present-day account of his doctrines is probably that of Partington.[4] The older German literature contains substantial exposition of some major doctrines, and among these texts I can recommend especially Gmelin, Sprengel, Kopp, and Haeser; and there is a recent Dutch monograph by Baumann.[5] Mani,[6] in his excellent recent book on the liver, translates some of Sylvius into German and gives a worthwhile discussion of some doctrinal points. For the present chapter I have relied on the Latin text, in the 1680 edition of the *Opera Omnia*.[7]

Sylvius did not provide any formal 'Institutes' of medicine as did Sennert, Riverius, or Hoffmann. His first major publication was a series of ten *Disputationes*, or dissertations, dealing largely with physio-

logical principles and concepts. These Disputations had originally been maintained by his students, between the years 1659 and 1663, but, in the fashion of the era, represented the professor's work. Gmelin gives the dates and names of the students who originally supported the doctrines.[8] When these works were collected into a single volume, they bore the title [in translation] 'A Decade of Medical Disputations, Comprising the Primary Natural Functions of the Human Body as well as the Nature of Fevers, derived from anatomical, practical, and chemical experiments.' This work presented some basic physiological background but not in a formal or systematic way.

His other major works, readily available in the various editions of his *Opera*, include an important *De Methodo Medendi*; and the massive *Praxeos Medicae Idea Nova*, which discussed various clinical diseases and pathological states and included much physiology and pathologic physiology. Only Book I of this work appeared during his lifetime; Books II and III and an extensive appendix were published posthumously. The various physiological concepts that weave in and out of the *Praxeos* repeat the ideas expounded in the *Disputationes*.

II

Authors like Sennert, Riverius, Hoffmann, and Boerhaave, who were highly systematic, provided an orderly survey of medical doctrine. In their *Institutes* they first took up *physiology*, which covered part of what we today mean by the term—the way the body functions. But more fundamentally, physiology discussed the *physis*, 'Nature', i.e., the world around us, particularly the animate world. We have seen how Riverius expounded the various first principles—the four ultimate elements and the primary qualities—and then built up his teachings from this basis. Van Helmont, although not as systematic as Riverius, and not providing any formal *Institutes*, nevertheless also wrote several treatises on the fundamentals of nature, in which he expounded his basic doctrines of water, ferments, and spirits.

Sylvius did nothing of this sort. Most of his writings were clinically orientated, quite closely concerned with concrete disease states. The ten *Disputationes* came closest to asserting his basic principles but are far removed from any orderly exposition such as we find in Riverius.

Indeed, Sylvius jumped into the middle of things, began with a discussion of digestion, and then proceeded to take up other systems of the body.

Before discussing his physiological principles I would like to take up what we call methodology—the way he approached the subject of evidence and validity, the critical acumen that he showed. Scattered through his expositions we find some occasional paragraphs which bear on these topics. He did not explicitly set down his principles of method, of validity, of evidence, of critical evaluation, but here and there he referred to them by indirection only. If we want to study the growth of critical awareness and its relations to scientific method, we must study the methodology implicit in Sylvius' writings.

We find, first of all, an opposition to dogmatism. Sylvius objected strongly to the claims of authority, to dependence on the unsupported teachings of anyone, ancient or modern. To no one, he said, is given the position of dictatorship in philosophy [i.e., natural science] or in the investigation of truth.[9] And he condemned anyone who asserted that the art of medicine could be derived from the dictatorial teachings [*dictatura et placitis*] of any ancient or modern writer. On the contrary, he said, whatever is good and assured in medicine has always been established and provided through experience alone, experience manifest to anyone.[10]

In such a passage he is implicitly condemning the rationalistic mode of achieving truth—i.e., using some assured premise as a starting place and then reaching new truth by logical inference. He refused to accept the teachings of ancient writers—their mere *ipse dixit*—as the basis for logical elaboration in medicine. He did not, however, seem to object to the process of inference itself, but rather to the dogmatic basis on which the inference might rest.

In his writings we find a prominent degree of what we can only call scepticism. That is, he insisted on the tentative nature of all his conclusions. The phraseology with which he expressed his doctrines was usually quite cautious. He frequently used the word *suspicamur* [we suspect or surmise][11] and *videtur* or *videntur* [it seems], to introduce his assertions. To such an extent did he carry this that he even distinguished between his own *suspiciones* and his *opiniones*. This rather subtle point we might render in English by making a contrast between 'suspicion' or 'idea' or 'surmise', and a more positive 'belief'.[12]

Experience teaches that men often go astray [*hominem quemvis saepius*

haesitare atque aberrare quotidiana testatur Experientia]. Not only medicine but all art is *conjectural*, not worthy of the appellation 'science'.[13] Sylvius held to the importance of *experience*, but experience required reason. In one passage he spoke of reason as the leader accompanied by experience [*Ratione duce ac comite experientia*].[14] And the necessary conjunction of reason and experience is a recurrent theme.

All mortals are fallible, he said, and may deceive or be subject to deception. Hence, no one, he declared, has the prerogative of offering a product of his own imagination [*aliquod cerebri sui commentum*] as the truth without a thorough testing through sensory perception or reasoning [*probationem sufficientem a Sensu integro, vel Ratione certa petendam*].[15] It is necessary to see repeatedly with your own eyes[16] before accepting assertions, while sufficient sensory evidence serves as warrant for acceptance. Elsewhere he declared, nothing in medicine or in the study of nature is to be accepted as true unless experience has shown it to be true or has confirmed it by actual perception [*confirmaret per sensus externos*]. Whatever the wisest and most subtle genius can think of, however plausible it is, ought still to be suspected of being false until experience, that master of truth, has shown it to be true, that is, to be actually observed in reality [*in rebus ipsis actu observari*].[17]

This view of the early 1660's gave lip service to experience but did not adequately show what constituted experience. There is the vague sense of 'seeing is believing', that somehow a doctrine must be 'manifest' to the 'senses'. Today, when we appreciate the complexities of what is loosely called 'perception', it is hard for us to enter into the 17th-century frame of mind which did not in any way analyse just what was being perceived, what was objective and what subjective, what was 'out there' and what merely imagined. Nor did Sylvius wrestle with the problems of criteria of truth, nor with problems of inference, nor with the knotty details of scientific methodology. He propounded a scepticism, that medical science was conjectural and probable only. He emphasized the importance of observation and condemned unsupported dogma—i.e., statements without concrete empirical evidence. But the crucial point—how much evidence and of what kind is required to support a given assertion—this he did not take up. How do you recognize or establish a *fact*? How do you confirm a theory? These problems quite escaped him. Indeed, he did not even perceive the existence of such problems.

He realized that science must search for causes, that in science the

analytical method begins with what is observed and then reasons backward to the causes thereof. The primary observations, i.e., the effects, we perceive through the senses and these are known first. Through reason we can then work backwards to the causes. Two points are of significance. First, he declared that causes can be made manifest only by reasoning [*causas, soli Rationi per Ratiocinationem patentes*]; and secondly, that the causes are less known—less assured—than are the effects, for they are known to us only subsequently and, so to speak, indirectly [*nobis minus quia posterius ac per consequentiam demum notas*].[18]

Sylvius did not set out to provide a critique of scientific investigation. In his rather unsystematic way he presented his own medical doctrines and he intercalated, here and there, ideas that related to proper method. If we inquire, to what degree he provided sound evidence for his own medical teachings or how cogently he reasoned from the data at hand, we find an extreme discrepancy. As we shall see, his medical assertions lacked evidence, his inferences lacked force. He did not practice what he preached, but is a fine example of the expression, 'Don't do what I do, but do what I say.' Although he claimed that his teachings were conjectural and tentative, he phrased them in dogmatic terms, with little trace of genuine doubts. He made declarations without providing evidence, and the evidence that he did offer was feeble in the extreme, sometimes resting only on analogy, or on unproven assertions of others. Often his own assertions represented only flights of fancy, having merely tenuous connexion with observation.

III

Sylvius began his first Disputation by taking up the subject of digestion. Food, he declared, is transformed into chyle. The process of chylification he considered to be one of fermentation, that is, the breaking up of a mixed substance into components. In general, separation can take place by fire, which is destructive, or by fermentation. The latter, requiring water as a medium, is the method by which salt is broken up, while fire transforms and alters oily substances—or oil. Water dissolves salt, fire alters oil. Fermentation must have sufficient water, a moderate heat, free access to air. The model of fermentation was the

production of wines and beers, but the same principles were at work in fermentation in the body.

Mastication, he indicated, prepares the food for fermentation which takes place in the stomach. The saliva, which contains the fermentative forces [*vires*], is composed of a large amount of water and small amounts of salt and spirit.[19] In the stomach the fermenting process is carried out and facilitated by the ingested water; by a moderate *fire* from the heart, communicated through the blood which the arteries carry to the stomach; by a free exit for gas in eructations through the oesophagus; and by a particular salt hidden [*latitans*] in the swallowed saliva.[20]

The 'spirituous' part of the saliva, he claimed, is not without use in promoting fermentation, as shown by the useful results from spirits of wine sometimes taken for that purpose. 'Spirits of wine'—what we call alcohol—was a spirituous substance sometimes useful in promoting digestion. Therefore, thought Sylvius, the spirituous component of saliva is also not without value in helping digestion. However, he did not demonstrate the existence of any such component in the saliva.

From the chyle the dregs or faeces must be separated off and the useful part pass into the lacteal veins in the intestine. The volatile spirits are eliminated by belching. Acid spirits are intimately joined to an alkaline salt and to an earthy substance, and are separated only with the greatest difficulty. The fermentation breaks up the components, and the fluid part which is useful to the body is separated off from the more solid portions which, not useful, are excreted.

Sylvius has substituted the word 'fermentation' for the Galenic 'faculty'. But in so doing he has claimed kinships between the processes in the body and chemical processes performed outside the body. This is a crucial difference, and yet so far as demonstrating what actually takes place in the body, Sylvius had not made much progress. He provided a point of view and not a body of information or of evidence. He asserted a faith, that the bodily activities depended on chemical activities, and he argued by analogy. And analogy, ultimately, depends on faith.

The purest parts of the foods, more fluid and more useful for producing blood, are separated by fermentation in the stomach and, as chyle, are then separated in the intestines from the more impure and thicker portions which pass into the faeces.[21] The separation [*secretio*] of the fluid from the more solid parts cannot take place by fermentation alone but requires some new alterations of a chemical nature. From the

fermented substances the component parts get separated and recombined,[22] but he did not provide details. However, it all takes place by chemical means and in this process the important factors are the bile and the pancreatic juices, and the combination of acid and alkali.

The pancreatic juice was discovered by Sylvius' pupil de Graaf[23] and was considered to be of acid nature. Sylvius believed that the pancreatic juice differed little or not at all from saliva.[24] The bile he considered to be an alkaline substance, containing abundant alkaline salt and 'subfatty' material [*subpinguis*] to which is added some oil, some volatile spirits, and a little water.

The alkaline salt in the bile helps overcome the viscidity, which arises from too much oil mixed with a little acid spirit and a small amount of earth. The chyle is diluted by the pancreatic juice. The acid spirit, diluted with water, destroys the viscidity and overcomes the force of the oil. The chyle separated from the faeces is filtered through the spongy membrane [*crustam*] of the intestine and passes into the lacteal veins by means of the peristaltic motion. Comprising the chyle are above all the volatile spirit of the food; a delicate oil, moderated [*temperatum*] by a small amount of alkaline salt; and a small quantity of acid spirit, diluted with water.[25]

The chyle enters the lacteal vessels, and through the thoracic duct reaches the left axillary vein where it pours into the bloodstream and is carried to the right heart. The blood of the superior vena cava thus contained both lymph and chyle contributed by the thoracic duct. In the right heart the innate heat—the *internus cordis ignis*—rarefies and kindles this heterogeneous material and promotes the mixture of the different elements. The blood, still imperfect, is then carried through the pulmonary artery into the lungs where it mingles with the inspired air which was a moderating function. Then entering the left auricle through the pulmonary veins, and from there the left ventricle, the blood is again kindled [*accenditur*] and rarefied by the same internal fire of the heart, and receives its ultimate 'perfection'.[26]

The blood, 'kindled' and 'rarefied' by the internal fire of the heart, was thus rendered more suitable for circulation. The circulation had important functions. It provided a continuous nourishment [*pabulo*] for the internal and indwelling fire of the heart which was the 'fountain of life, and the primary bond of the soul and the body'.[27] Then, it maintained vitality in the different parts of the body.

The blood flowing out of the heart is, he thought, warmer than

the venous blood entering the heart, and is heated by the internal fire. The latter did not get exhausted and the reason for this he could only illustrate by analogy—a torch or candle can set afire innumerable other torches or candles, without losing its own flame. The innate heat could continue and distribute its essential quality to new blood as it enters.[28]

A moderately acid spirit rarefies the blood and coagulates the fatty and oily parts. As a result, the blood will better withstand the internal fire of the heart.[29] This seems to mean that the acid spirit allows the new blood to have a smoother and more long-lasting 'ignition' by the fire in the heart. Meanwhile, by the innate fire the components of the blood are better mixed.[30]

Sylvius did not accord the liver a part in the process of sanguification. The functions of the liver are discussed later.

The brain separated off the animal spirits from the blood. The carotid and cervical arteries carried the blood from the heart into the substance of the brain and cerebellum. This blood nourished and 'vivified' the organ but also furnished the material for the elaboration of the animal spirits. The most spirituous part of the blood penetrated through the pores of the arteriolar capillaries in the cortex and thence into the underlying white substance. In this passage it was freed of its aqueous component and became purified, analogous to the way that 'spirit of wine' is 'rectified' from its phlegm. The cortex of cerebrum and cerebellum was thus a mechanism for separating out the finest animal spirits from the blood, and purifying them.[31]

Nerves carry the animal spirit to all parts of the body, but if not carried away in the nerves the spirit could accumulate through the entire brain. The animal spirit that passed into the nerves was not used up entirely, nor did it vanish, but a substantial part passed over into the lymphatic vessels and from there returned to the blood. Thus, Sylvius held to a circulation of the animal spirits: from the blood to the brain, from the brain through the nerves, from the nerves to the lymphatics and back into the blood.[32] The watery part of the blood, after the cerebral cortex had separated off the spirituous part, formed the fluid in the meninges and the ventricles.

The animal spirits served both the external and the internal senses, as well as the processes of imagination, reasoning, judgement, and memory. In addition, the animal spirits, the most pure and subtle of substances in nature[33], communicated the affections of the mind to the

body and the affections of the body to the mind. Furthermore, the animal spirits served the 'animal motion' through muscular fibres, and brought about peristaltic movements. Muscular movement, both voluntary and involuntary, was carried on by means of animal spirits.

All glands have the function of elaborating humours which are useful and necessary for the body.[34] He distinguished two principal types of glands, the conglobate [lymph nodes] and the conglomerate [such as the salivary glands and the pancreas].

The saliva, prepared in the salivary glands, consists of a large amount of water, a small amount of volatile spirits, a very small amount of alkaline salt with oil, and of acid spirit. The saliva is very similar, he thought, to the pancreatic juice.

The lymph, prepared from the watery part of the blood, mingled with the animal spirits carried to it.[35] These, not altogether consumed in their own functions, had flowed back for a re-use in the body.

The saliva moistens the mouth and the throat. Continuously swallowed, it promotes the preparation of a new ferment in the stomach.[36] Sylvius did not distinguish between the saliva produced in the mouth and the intrinsic glandular product in the stomach, but he held that the saliva acted in both the mouth and the stomach. It promoted the fermentation of the food material. The fermentation in the stomach had its beginning in the mouth.

The pancreatic juice was deemed not very different from the saliva. It had as its function the separation of the chyle from the faeces. The lymph, Sylvius thought, might have a function of helping the blood and the chyle to unite more easily.[37] The fluid prepared in the 'renal glands' [i.e., adrenals], from the blood and the animal spirit, had a regulatory function on the blood. He believed that the blood was rendered less fluid as it passed through the kidneys which removed the urine. The renal glands, he believed, prepare a humour which keeps the blood from coagulating or agglutinating.[38]

The bile played a central role in Sylvius' physiology. In the sixth Disputation he discussed the anatomy and physiology of the liver and the biliary system. Bile was prepared from the blood, but not in the liver. Instead, he claimed, it is prepared in the gallbladder,[39] and then flows down, to be divided into two portions. One portion enters the duodenum, to take part in the process of digestion, while the other portion enters the liver through the hepatic duct. In the liver the

purified bile enters the blood stream in order to take part in complex activities. The liver provides a framework of vessels, in contact with the ducts, so that the bile could enter the blood stream smoothly. Mani, in his recent volume on the liver, discussed this aspect of Sylvius' system with great clarity.[40]

The bile consisted of a great deal of alkaline salt, a small amount of water, and a moderate quantity of oil and volatile spirit. Its composition explained its function. By virtue of its salt, it readily mixes with water. It could ignite, a property due to the oil. It has a penetrating quality, by virtue of the volatile spirit. The fluidity derives from its water. If the ingredients were properly mingled, the bile was 'well disposed', but if the proportions were disturbed, unfortunate and bad effects could follow. The bile acted on the fermented food and helped separate the chyle from the faeces, as well as from the intestinal phlegm.[41]

In the portal vein and the branches of the vena cava, the blood flows back from various organs, particularly the spleen. Here it had been additionally perfected [*plus quam perfectum*] by animal spirit, and thus rendered more suitable for taking up the bile. The bile has a key role in supporting the vital fire to be found in the right ventricle of the heart.[42]

Sylvius enumerated the functions of the liver.[43] As already mentioned, it furnishes a framework for the delicate branches of the vena cava, portal vein and hepatic duct as they spread into a capillary network. And then, by its gentle heat the liver promotes the mixture of the bile and the blood flowing through the veins.

The doctrines of Sylvius diverge remarkably from the teachings of his predecessors. It is strange that he ignored the various functions that his predecessors had attributed to the liver. Perhaps his own doctrines of the importance of the bile coloured his views. If we grant that bile has extreme significance in maintaining life, the passage of the bile into the blood and the smooth mixture would take an important place in the theoretical structure.

His pre-existing views of the importance of bile led Sylvius to his speculations concerning the functions of the liver. This was the dominant principle, and it controlled the way in which he regarded the anatomy. We must not, however, feel that his doctrines were entirely fanciful. He had what he considered concrete evidence, namely, dissections and experiments whereby he 'proved' a patent pathway

between the gall bladder and the arterial branches of the liver. His technique was the insufflation of air.[44] However weak the evidence seems to us, to him it appeared valid. The temper of the era conditioned his mind to accept it.

Respiration was essential for life. Sylvius believed that life depends on the vital heat (or, as he phrased it, the continuity of the fire kindled in the heart[45]), and this heat is distributed through the body by the blood. The vital fire, the essence of life, in turn depends on certain other factors, which we shall take up later in more detail. Sylvius believed that fire was material, and that fiery particles were freed from their entanglements and, by gently kindling the fatty part of the blood, produce the heat manifest to our senses.[46] The inspiration of air, thought Sylvius, had a moderating function on the fire, while the expiration expels the heated vapour.[47] The relationship to Galenic thought is obvious.

The lungs stand in special anatomical and functional relations to the heart. Animals with lungs have a higher heat than those without lungs. And respiration, he noted, is increased or decreased according to the increase or decrease of heat in the heart, i.e., respiratory rate seemed correlated with the degree of heat. Increased heat in the heart was tempered by the inspired air.

Noting the increased respiratory rate that accompanies increased heart action, Sylvius had assumed that the more rapid heart action indicated an increased production of heat. Fire in a furnace, when it increases or decreases, requires a greater or lesser amount of air. Similarly, if in the body the vital flames increase or diminish, the inspiration and expiration correspondingly increase or diminish. Using further analogy, he pointed out that a furnace fire must have free access of air to the lower part and free outward passage of air from the upper part or it will go out. In similar fashion he pointed to the phenomenon of respiration; the vital fire will go out if respiration—the ingress and egress of air—is cut off.[48]

Since the inspired air tempered the heat of the blood, something must pass from the air to the blood, something opposite to what the blood receives in the heart. Now nothing approaches salt in its condensing power and ability to check heat. Therefore, Sylvius 'suspected' that some pure and simple salt is dispersed in the air. This salt occurs

at its purest in nitre. He concluded that in the lungs the inspired air, by virtue of its pure salt, serves to condense the blood previously rarefied in the right ventricle, and also to check the increased heat that has been aroused.[49]

Lymph played an important role in his doctrines, but since its composition and function were a matter of dispute and no positive data were at hand, Sylvius indulged in conjecture based on analogy. The lymph had its own system of vessels, closely associated with the little 'glands' which he called 'conglobate' (in distinction from the 'conglomerate' glands which, composed of many small lobular masses, produced secretions such as saliva or pancreatic juice). The lymph all flowed into the thoracic duct and, emptying into the subclavian vein, ultimately rejoined the blood stream. If the lymph were a waste product, like urine, it would have been excreted and not returned to the blood.[50] Since it was not eliminated, it must have some special usefulness.

The chyle, absorbed from the intestines, flows through the thoracic duct, mingles with the lymph, and then passes into the blood stream. Consequently, Sylvius 'suspected' that the lymph promotes the more ready mixture of chyle and blood.[51]

But clearly there were other functions. Lymph, which flowed centripetally, is produced in most parts of the body, and lymph formation in muscles and other organs is not a prime but only a secondary function of these organs. The conglobate glands [lymph nodes] had the primary function of preparing a special lymph [*peculiarem lympham*] analogous to the secretions that the conglomerate glands prepared for discharge through their ducts.[52]

All lymph, flowing from different organs, need not be the same. From some parts it seemed to be a 'superfluity' that had not been utilized and which was returning to the circulation. But from other organs it seemed to be specially prepared or elaborated,[53] i.e., not merely an overflow.

Whence did lymph arise? Sylvius gave the ready answer—From the animal spirits. He indicated an analogy. Blood is carried to the various organs, which utilized any material necessary for nutrition, while the remainder of the blood flows back through the veins. Similarly, the animal spirits, carried to various parts through the nerves, are more

abundant than the organ function required. Consequently, the residue of the animal spirits flows back through the lymphatics. Like the liver, the conglobate glands have a 'mixing' function. From the blood the glands separate out an acid spirit and this was then mixed with the animal spirits brought by the nerves.[54]

To clarify the role of acid spirits Sylvius pointed to various ailments —cramps, vomiting, nausea, flatulence, even epileptic attacks—that 'beyond doubt' [*dubio procul*] had their origin in the effervescence of acid spirits and bile.[55]

Whatever in nature is sharp and biting, apart from fire, is either an acid spirit or an alkaline salt. Now these acrid substances derive their 'acrimony' from fire. Acid and alkali can be prepared from saline material by the force of fire. No burning fire as such exists in the body, but the acid material [which is in a compounded state] gets freed from its bonds, namely, the oil and volatile spirits which temper it.[56]

The origin of the acid posed a problem. Acid material existed in the blood, as evidenced by coagulation of blood outside the vessels.[57] Sylvius employed some oblique reasoning. It was well known that acid, when added to blood, would coagulate it. And obviously, blood removed from the body will spontaneously coagulate. Therefore, he apparently reasoned, the spontaneously occurring coagulation must be due to acid.

The acrimony of acid spirits was tempered by volatile spirits and also by fatty substances. He believed that acid spirit and volatile spirit unite very readily. So do alkaline salt and oil. However, volatile spirit does not combine readily with alkaline salt, and acid spirit and oil have the most difficulty in combining.[58]

In the conglobate glands, he claimed, the volatile spirit joins with acid spirit; in the pancreas, the acid spirit unites with oil; and in the maxillary glands, oil unites with both acid and volatile spirit. Various diseases arise from defects of these components—e.g., viscid humours that acquire a stony consistency come especially from acid spirit. Catarrh, coryza, sneezing and cough arise from an inadequately tempered acid spirit.[59]

In the lymph the volatile (i.e., animal) spirit was the more abundant component, acid spirit the less. For its function the lymph prevents the excessive consumption of the animal spirits and returns the residue to the blood. Lymph, containing some acid spirit, and entering the right heart from above, meets the blood coming from the liver which

had been impregnated with bile. They meet in the right side of the heart, and in health all is gentle, bland, and favourable to nature. But where there is some disturbance [*vitiata sunt quaedam*] then a more or less harmful effervescence takes place, hostile to nature.[60]

IV

The nature of fever puzzled 17th century physicians but nevertheless they had to find some criterion to identify the condition. The characteristic mark of fever Sylvius considered to be the 'preternatural' rapidity of the pulse. By this terminology he tried to distinguish between physiological and pathological states. Thus, he excluded the increased heart rate that occurred in exercise or emotion, or other 'non-naturals'. If, however, there were an 'error' in the non-naturals, or an 'abuse', with harm to the body, pain, and impaired function, then an increased pulse would be 'preternatural'[61] or pathological. The possibility that the essence of fever might lie in an increased heat of the whole body he considered carefully, but rejected. Pathognomonic signs must be present in all cases of the disease and be universally present only in the disease. In fever, as he conceived the term, increased heat did not meet these requirements.

Sylvius had to explain the mechanisms of the increased heart rate, and this, in turn, related to basic cardiac physiology. He rejected any 'pulsific faculty' but explained the pulse action by physical and chemical processes, by the dilatation and contraction of the heart and arteries.

> The dilatation of the ventricle [diastole] came about partly from the increased amount of blood entering the cavity and partly from the rarefaction of the blood. That is, the blood, as it became rarefied by heat, increased in volume.[62] As evidence he pointed to common experience—heated fluids expand and became rarefied, and 'no one will doubt' that a comparable change can take place in the ventricle.[63] For this rarefaction or expansion to take place the heart must furnish an extra amount of heat to the entering blood. That this does in fact happen he considered to be a fact of experience. Thus, if a finger is inserted into the ventricle, he said, a greater heat will be perceived there than in any other part of the body.[64] This statement, really a cornerstone of his entire system, he did not subject to any systematic

investigation. The empirical evidence consisted only of the sensation of his finger. We are not told how many times the experiment was performed, nor under what conditions, nor what other parts of the body were studied, nor what might be possible sources of error, nor what controls were adopted—in brief, he had no conception of what we today consider to be the canons of experiment. The noteworthy feature, however, is not that he failed to appreciate niceties of experimentation but that his brief 'experiment' carried conviction and demanded no critical evaluation.

The actual contraction of the ventricle resulted from the action of the heart muscle which had been stimulated by the expansion of the blood [*a rarefacto . . . sanguine factam irritationem*].[65] The contraction of the heart and the compression of the ventricle, which took place in accordance with mechanical laws[66], ejected blood into the arteries and dilated them. Sylvius firmly asserted that the arteries dilate because they were filled with blood, and not the reverse, that they filled up because they first dilated. He thus rejected any 'bellows' theory of arterial expansion.[67] The contraction of the arteries depends on the transverse 'fibres' of the artery which have a 'natural motion', i.e., a contractile and constrictive activity like the peristaltic motion of the intestines, dependent in turn on the flow of animal spirits filling the fibres.[68]

He summarized his views thus far: the pathognomonic sign of fever is the preternatural rapidity of the pulse; the pulse consists of the dilatation and contraction of the cardiac ventricles and the arteries; the expansion of the heart is due to the rarefaction of the blood which spreads [*Sanguine explicanti*] the walls; contraction results from muscular action, from the flow of animal spirits; the dilatation of the artery is due to the flow of blood forced out by the ventricular contraction; the arterial constriction occurs as the animal spirits fill the annular fibres.[69]

Thus far he has been dealing with what we might call the mechanical factors of the heart and pulse, but all this, in a sense, merely prepares us for the basic chemical explanation that forms the crux of his system.

Fire played a central role. To indicate how it arose and received its support, he demanded demonstration and argument, not mere assertion based on authority.[70] The vital fire did not really resemble ordinary flame. Obviously there was no lucent flame in the heart, nor anything analogous to a wick, nor any fatty or oily substance apart from the blood. Nor did the increase in heat that occurred in the heart take place

in the fashion of burning oil or wax or wood.[71] All derived from the blood and Sylvius tried to explicate the mechanisms.

He rested his argument on certain observed data. The blood entered the heart from two separate sources, the superior and inferior venae cavae. The blood from these two sources, he thought, has quite distinct compositions. The blood from the inferior vena cava includes the material from the liver, particularly bile. The blood that descended into the heart contains lymph, entering via the thoracic duct; chyle, produced by digestion of food; and also (when no food was eaten) the fluid produced in the intestine from combination of bile, pancreatic juice, and the constantly swallowed saliva.[72] Other contents of the blood stream he did not believe had significance. The important components, then, were the 'descending' lymph and chyle and the 'ascending' bile, and to these the mechanism of vital fire could be assigned.

The bile and lymph have certain special properties. The bile abounds in alkaline salt and volatile and oily material. On the addition of an acid spirit there is 'more or less' effervescence and various changes occur according to the kind of acid spirits.[73]

In contrast to the bile, lymph constantly flowing to the heart could not be collected pure in any significant amount, and its properties could not be well investigated. It seemed to have a sour taste. This, it should be noted, was a characteristic property of acids. Sylvius, so far as I can tell, did not use indicators, and Robert Boyle's experiments with colour changes were still not yet published.[74] Nevertheless, Sylvius believed that the lymph is 'subacid'.[75] The acid properties of lymph he also inferred from other observations—the catarrhal affections attributed to lymph (i.e., the irritant properties which produced catarrh, pointing to an acid); the diseases of lymph glands, especially cancer. These diseases served as the signs of acidity (i.e., erosion), and there was thus indirect evidence that the lymph had an acid nature.

By a comparable chain of indirect reasoning he indicated the acid nature of the pancreatic juice. He pointed to the eructations of acid and bilious material, especially in infants, and queried, whence arose its sharpness? His answer provides an interesting exercise in reasoning. The 'sharpness' is to be sought perhaps in impaired [*vitioso*] pancreatic juice; or perhaps in saliva, or in other humour dripping down from the head; or else in medicines; or in poisons swallowed and carried to the intestines.[76]

He examined these possibilities by logical scrutiny. In infants

nourished by sweet milk or broth, no right-minded person [*sanae mentis*] could attribute this sharpness to food, medicines, or poisons. Nor could the acidity be attributed to saliva, for then the mouth and esophagus would be irritated.[77] By inference, therefore, the sharpness of the eructations derives from the bad state of the pancreatic juice, too acid and sharp. In its normal state the pancreatic juice is subacid and tempered. Then, reasoning still further, Sylvius indicated that the pancreas must separate out the acid fluid from the blood and by the animal spirits temper and render it more bland.[78]

If conglomerate glands like the pancreas removed a subacid fluid from the blood and tempered it, Sylvius did not see why the conglobate glands should not act similarly, to separate the acid from the blood and temper it with animal spirits. These spirits, as we saw, are in part carried by the nerves, in part come from muscles and other organs by way of the lymph. Although he called this a 'conjecture,'[79] he saw it confirmed by various diseases where the conglobate [lymph] glands were involved, as in coryza and catarrh.

Thus his reasoning confirmed the subacid nature of normal lymph, which some had already asserted on the basis of taste alone. All this, however devious it seems, is preliminary to expounding the action of the vital fire. If in the heart the subacid lymph in the descending blood mixes with the bile contained in the ascending blood, will not this mixture 'necessarily' give rise to an effervescence, bland and favourable to nature?[80] Sylvius propounded this in the form of a question, but obviously it was rhetorical only.

He adduced evidence from *in vitro* experiments.[81] An acid spirit is produced when fire acts strongly on a salt, enclosed in a vessel. An alkali remains in the ashes. The acid requires a closed vessel, the alkali an open flame. Since each gets prepared through the action of fire, he concluded that abundant fire is intimately joined to both. That is, fire particles formed an intrinsic part of both acid and alkali. The two substances, having similar components, mix readily and a new reaction takes place. The fire particles, previously enclosed in both components, now get liberated from the tempering bonds, begin to act on all parts of the mixture, agitating and rarefying it, even in part flying off in a perceptible fashion. This process, the agitation and ebullition with manifest heat, *produced by the freeing of fire*, is called effervescence.

In the heart[82] the fire particles [*ignis*] leave their twin 'home'—i.e., the acid and the alkali—and insinuate themselves into whatever parts

of the blood are more suitable to their nature, as, the oily part of the blood. Oil is particularly appropriate for receiving fire particles. Is it any wonder, asked Sylvius rhetorically, if the oily part mixes with the fire and undergoes combustion [*ipsum accendat*]; and the fire arising from the effervescing bile and lymph expands the rest of the blood? He gave the analogy of acid spirit of vitriol poured on oily spirit of turpentine and a volatile salt.

If the bile and lymph are well tempered the resulting effervescence is bland and useful to the body. If either or both are less tempered, a sharper effervescence is produced, harmful to nature and giving rise to one or another kind of fever.[83]

To summarize his views. The essence of fevers, we have seen, was a preternatural rapidity of the pulse which, in turn, reflected the activity of the heart. He described the mechanism of the normal heart-beat as dilatation from expansion of the blood, followed by contraction resulting from muscular action. The heart was the centre of vital heat. This vital heat, in turn, came from the fire particles shut up in acids and alkalis contained in the blood; the lymph and pancreatic juice were the principal sources of acid; the bile, of alkali. The lymph, chyle, and absorbed intestinal juice (including pancreatic juice) reached the heart from the superior vena cava; the bile, disseminated into the blood in the liver, came to the heart in the inferior vena cava. Normal components produced a normal effervescence, a normal vital heat, and normal pulse rate. If the pulse were abnormal, as in fever, this in turn must have resulted from an abnormal composition of the lymph, pancreatic juice, and bile. Too much acidity, too great an alkalinity, would liberate more fire particles, with more intense action on the heart and therefore on the pulse. The reaction on the heart resulting from an abnormal effervescence was rather distinct from other types of reaction which excess acid could produce elsewhere in the body.

The topic of excess acid and alkali, and the 'acrids', and their place in inflammation and other disease states, received particular clinical elaboration in the teachings of Boerhaave, whose concepts of acid and alkali acrimonies were extremely influential. Hoffman, in his *Fundamenta*, employed these concepts to some extent, and might be regarded as a transition to the more complicated formulation of Boerhaave.

Sylvius was a physician, a practitioner of medicine and not a philosopher. He had no concern with abstract metaphysics nor with basic methodologic principles. Manifesting the trend of his times he

was a 'modernist' in the 17th century sense, who turned his back on Galen and paid attention to the new discoveries that the anatomists, physiologists, and chemists of this era were making. Sylvius used these discoveries as the groundwork on which to erect a system, whereby he could place all medical data into neat pigeonholes. He could give a logical answer to any question by referring to first principles which he believed were empirically based: acids and alkalis were data of direct experience.

This use of acids and alkalis, as explanatory principles, linked up bodily processes with the phenomena of the inorganic world. Once we grant that health depends on a particular relation of acid and alkali, then any deviation from health could be explained by abnormality of these substances—e.g., by a different composition or proportion or interaction, or some such variation. But just *how* different? What *was* the composition? Wherein lay the actual divergences? Sylvius was unable to answer any such questions, nor did he really try. His doctrine rested on conjecture and inference, on logic rather than on really sound observation.

The experiments on which he based his doctrines were rather feeble, his credulity was great, and his critical acumen low. He made a small amount of observation—faulty at that—serve as basis for a large amount of theory. Despite all these faults, it exerted great influence. He was in fashion, adapting to medicine the exciting new developments in science. While his 'system' had a great many gaps, he did provide a series of interlocking concepts, integrated one with the other, and permitted inferences regarding therapy. This was typical of a 'rationalist' system. Furthermore, his devotion to chemistry and the way he embodied chemical doctrine in his theoretical explanations gave considerable support to the more empirically based chemical therapeutics that competed with the so-called 'Galenicals'.

Sylvius was severely criticized by 18th century writers, for the explanations he offered did not explain well enough, while new discoveries and new experiments rendered his theories rather untenable. Nevertheless, he played an important part in 17th century medical progress.

SYDENHAM

Thomas Sydenham (1624–1689) has a definite popular 'image' in medical history as a physician who insisted on facts and not speculation, who believed in observation and rejected theoretical elaboration. His designation as the English Hippocrates showed the esteem in which he was held. In this connection Temkin[1] has given us an admirable study of Sydenham and Hippocrates, the points of similarity and the points of divergence.

We have seen that Sylvius avoided metaphysics and tried to base medicine on chemistry. In so doing he dogmatically adopted chemical concepts and accepted inferences with little evidence to back them up. We see him as part of the slow progress which adopted the trimmings of experimental science but without the critical evaluation of data.

Sydenham also condemned metaphysical speculation and prided himself on his powers of observation, believing that he remained quite close to fact. Whereas Sylvius fell back on basic chemical concepts that were quite far removed from observation, Sydenham held to 'facts'. He was the first great medical follower of Francis Bacon, who believed that the road to scientific progress lay through natural history, and accumulation of data. This is closely related to what we call empiricism which, in the history of thought, gets contrasted with rationalism.

Behind this contrast—which goes back to the ancient Greeks—there lies the assumption of two distinct mental activities, the one called perception and the other called reasoning. The one receives impressions of what is 'out there'; the other makes abstraction from what has been 'received' and then produces inferences and logical elaboration. The observational aspects were designated as empirical, the abstractions, and inferences were called rational.

The distinction, in one sense, is entirely arbitrary, for we cannot 'observe' without some sort of rational framework; and we cannot rationalize without an observational substrate. The two interpenetrate thoroughly and intimately. In another sense, however, the distinction is valid if it indicates a matter of degree, depending on the extent to which we attend to particular observations or to abstractions and generalizations. We always do both, but we may emphasize one at the expense of the other.

Sylvius, starting with a few observations, rapidly proceeded to what we can call 'unsupported generalizations'. These he set up as first principles of a system, and used them to support various further inferences. Sydenham, on the other hand, constantly relied on generalizations and on many unwitting assumptions, but he did avoid soaring flights to highly abstract principles. Study of his works indicates particularly well how observation and reason interpenetrate and how the observational component can receive special emphasis.

As a follower of Bacon, Sydenham believed in collecting individual observations and in abstracting and generalizing them to form true statements about whole classes. Diseases, he thought, could be divided into real classes, each identifiable by appropriate marks. Virtually every commentator on Sydenham has quoted the famous passage in which he compared diseases with plants, and declared[2] that all diseases should be reduced to species 'with the same care which we see exhibited by botanists in their phytologies'. Diseases represented naturally occurring classes. To identify them, the physician, like the naturalist, must note 'the clear and natural phenomena' in minute and accurate details.[3] He should 'enumerate the peculiar and constant phenomena apart from the accidental and adventitious ones'.[4] All this, of course, means starting with concrete observations and by abstraction progressing rapidly to whole classes. This abstraction and generalization is a 'rational' activity.

In this sense, students of medicine must not only divide diseases into classes or species but also make general statements about the functional behaviour of disease, e.g., the response to therapy, or the appearance of various diagnostic or prognostic signs. The classical examples of such general statements we find in the *Aphorisms* of Hippocrates: 'Drinking strong wine cures hunger' or 'Acute diseases come to an crisis in fourteen days' or 'Spasm supervening on a wound is fatal'.[5] Statements of this character are indeed generalizations, but they are quite close to concrete observations and I designate them as 'low-level generalizations'. Other generalizations, however, deal with far more abstract entities, as in the doctrine of the four primary qualities, and rapidly leave concrete experience behind, to become metaphysical. These generalizations, far from being 'low level', are 'speculative'.

The significant difference between 'rationalists' and 'empiricists' concerns the degree to which they relate with concrete observations.[6] Both start with experience but the rationalist goes much more rapidly

into abstractions, the abstractions are more rarefied, and the subsequent contact with experience much less frequent. The rationalist can end up with 'first principles' of highly abstract character, utterly remote from perceptual experience and deserving the term 'speculative'.

Francis Bacon stressed the need for generalizations but he wanted them to derive from cautious step-wise induction and gradual progressive abstraction, with each step in the progression maintaining relationship to sense perception. He condemned soaring flights of inference that quickly left sense data far behind. We can say that the empiricist has his principal concern with the more proximal derivations from experience, while the rationalist concerns himself with the more remote derivations.

Sydenham expressly condemned the flight to abstract principles. To be sure, men do seek such abstractions. After observing nature, their 'understanding' tends to frame to itself some hypothesis, and to lay 'a foundation whereon to establish all its reasonings'.[7] Learned men in times past sought out the hidden causes of disease and the 'unperceptible tools' whereby nature worked. But excessive concern with the abstract principles and the logical implications and derivatives thereof 'has at last but confined and narrowed mens [*sic*] thoughts, and used their understanding with fine but useless speculations, and diverted their enquirys from the true and advantagious knowledg [*sic*] of things'.[8]

Like Bacon, Sydenham objected to soaring abstractions because the alleged first principles were insubstantial; mere cobwebs, spun out of the inner consciousness; mere fancies and speculations, not hard reality —i.e., not having adequate connexions with observational data. Logical inferences that rested too much on speculation could not advance knowledge. In regard to medicine Sydenham objected to positing 'fundamentall maximes [i.e., "first principles"] and from whence drawing consequence'.[8] Although this would reduce medicine 'into the regular forme of a science' it would only 'enlarge the art of talkeing' and lay 'a foundation for endless disputes'.[8] He compared it to walking up and down a thick wood overgrown with briars, and trying to draw a map. Speculative theorems do not advance physick.

I would indicate three points of significance. First, there is the idea that science is a discipline which rested on first principles. Sydenham had no theoretical objections to this notion. Second, he objected to the lack of proof—i.e., the speculative character—of the alleged first principles. And third, he disagreed with the basic Aristotelian concepts

regarding the importance of *knowing*. For Sydenham *doing* was far more important. Instead of verbalisms he wanted practical results and in medicine he condemned any stress on first principles because they were only speculative and did not lead to practical results.

Here we see a difference between Sydenham and Bacon. Bacon felt that sound first principles would closely integrate with practical concrete activity. Good theory led to mastery of nature. Scientists could conduct experiments of *fruit* which yield immediate practical benefits, but they could also conduct experiments of *light*, which illuminate the first principles of nature, even though no practical benefit immediately arises. Bacon had the implicit faith that proper light would yield fruit. Sydenham, however, was primarily concerned with practical effects and he had derogatory opinions about theory and investigation that did not contribute to practical therapeutic benefit.

In a recent interesting paper bearing on this point, Wolfe[9] studied the attitudes of Sydenham and Locke toward anatomy. Wolfe raised the point why microscopy, for example, did not make a wide appeal among 17th century physicians, and he implicated Sydenham as one who had no interest in the 'anatomization' of nature. He pictured Sydenham as a skeptic and provided many reasons for that attitude. I would, however, offer rather different reasons why Sydenham was averse to fine anatomical studies and to precise investigations of natural phenomena, independent of practical utility. The answer, I believe, lies in the life of Sydenham and his background.

II

By 17th century standards, Sydenham's education was quite defective. He entered Oxford in 1642, when but 17 years of age, but after only a few months left to engage in military service. He and his entire family supported the anti-Royalist forces in the struggle against Charles I. Young Sydenham saw a lot of military action. He returned to Oxford in 1647 at a time when the Puritans were replacing the Royalist faculty and conditions were hardly conducive to high scholarship. In less than a year he was granted the Bachelor of Medicine degree by fiat. He had a few more years at the University, including an appointment as Senior Bursar, but how much this helped his medical knowledge is not apparent.

During this period a great array of scientific talent assembled at Oxford, and created the informal group that eventually became the Royal Society of London. The story has been many times told.[10] Significant for our purpose was the failure of Sydenham to take any part, or indeed, any interest in this great development of natural science. Sydenham was not in any way an experimental scientist. He lacked both knowledge and interest, and, we may safely assume, ability. Certainly he had an unrivalled opportunity to get in on the ground floor, so to speak, of scientific investigation, but he did not avail himself of the chance. He ignored the new and, at the same time, had negligible background in classical learning. Yet he had chosen medicine as a profession; and as a highly capable person he created his own rather unique course.

Sydenham set himself the task of observing nature. Often this term was used as a personalized agent that regulated affairs with intelligence. He rejected all such senses and used the term to mean only 'the whole complication of natural causes; causes which in themselves are brute and irrational.'[11] Nature for him, thus, was the totality of events according to a strict impersonal causal nexus (but this totality of events did exhibit an intrinsic rationality that, in accordance with 17th century doctrine, stemmed from God the Creator).

Natural history, which describes nature, does not concern itself with ultimate first principles. It may provide the data for eventual derivation of first principles, but the naturalist, as such, does not try to find them. We can contrast natural history with natural philosophy. The latter seeks the ultimate underlying principles, the former only the data and the proximal generalizations therefrom. For Sydenham a philosophy of medicine 'consists in working out the histories of diseases, and applying the remedies which may dispel them; and Experience is the sole guide'.[12] In observing and defining the disease, as well as in treating patients, Sydenham was a naturalist. He had the explicit task of making observations, of collecting and analysing them so that proximate generalizations could occur. He wanted to avoid first principles entirely, since they represented speculation and fancy.

In the production of disease, nature was 'consistent and uniform' or, as we might say today, nature exhibited her phenomena in patterns.

'The self same phenomena that you would observe in the sickness of a Socrates you would observe in the sickness of a simpleton.'[13] Just as 'the universal characters of a plant are extended to every individual of the species', so too the diseases have universal characters that apply to the individual patient. By studying the individual the physician may find the pattern. He must find the essential features and distinguish them from the accidental or unimportant features.

While natural history actually deals with classes, nevertheless the practicing physician must treat particular sick patients. How best to acquire the proper therapeutic knowledge? Finding the proper treatment involved careful study. Any sound *methodus medendi* represented 'a line of practice which has been based and built upon a sufficient number of experiments, and has in that manner been proved competent to the cure of this or that disease. I by no means am satisfied with the record of a few successful operations, either of the doctor or the drug. I require that they be shown to succeed universally, or at least under such circumstances'. Empirical truth did not emerge from single instances. To establish empirical truth we must establish some degree of universality and define the relevant circumstances. To do this we must attend diligently to particular cases, so that 'by a long continuance and a frequent repetition' we can establish a *methodus medendi*.[14]

Single instances of therapeutic success he recognized as fallible. Sydenham wanted to test efficacy 'by innumerable experiments' and 'weigh, in respect to both the patient and the practice, innumerable circumstances', before he could 'derive any benefit from such a solitary observation'.[15]

A sound *methodus medendi* must rest on a knowledge of causes. Sydenham distinguished various categories of causes. Some we can actually know—those that are 'proximate, immediate, and conjunct . . . of which the evidence is certain'.[16] We should seek the causes that are immediate and capable of immediate and direct verification, those 'which appeal to the understanding at once; which are revealed by either the testimony of our senses, or by anatomical observations of long standing'.[17] He wanted to find the *immediate* cause which produced a particular disease, for then he could find the means by which it could be cured. The knowledge of immediate or proximal causes would enable us to diagnose and prognosticate, and carry out satisfactory cures.

The opposite of 'proximate causes' were 'remote causes', which for

him were 'altogether incomprehensible and inscrutable'.[18] These represented the speculative concepts which he rejected. For example in a pleurisy we do not understand 'the vicious crasis, and the incoherent texture of blood which is the primary cause of the disease', and search into such remote causes would be 'idle and unprofitable'. But, he thought, if we know the immediate cause of the disease we can effect a cure.

Sydenham would accept causation which depended on the 'testimony of our senses'. The senses provided 'obvious' relationships of disease, such as a trauma followed by swelling; or an exposure to cold followed by a fever; or else the immediate conjunction of medication and response, such as the vomiting that followed an emetic, or the evacuation after a purgative. The relationships here seem immediate and direct in contrast to those that depended on chains of inference and abstract concepts.

He would also accept explanations depending on 'anatomical observations.' The Latin reads, *anatomicis observationibus ab aliis jampridem factis*. Sydenham was trying to indicate his reliance on the anatomical observations which others had long since made and which had long been accepted.[19] This referred to gross anatomy only. Now Sydenham was not himself an anatomist, and he showed no interest in the new structures that the 17th century anatomists were discovering, nor in the 'living anatomy' of the physiologists. Why, then, did he emphasize anatomical observations in which he had neither interest nor competence? I offer this answer: that his insistence on 'anatomical observation' was his means of making acceptable certain hypothetical factors in medicine. I am referring especially to the humours, which were, in a sense, matters of observation. To some extent he could *see* the blood, phlegm, and bile. But in addition to what he could see, he attributed to the humours many other properties and activities which were by no means facts of observation. Yet the whole humoral theory had long since been accepted by anatomists and had for Sydenham the validity of direct observation, so far as explanatory force was concerned. He did not face the hypothetical nature of humoral concepts, because 'hypothesis' was a bad word, while the humours had an established status.

Contrasting with gross anatomy was microscopic anatomy, in which Sydenham had little faith. Wolfe[20] has discussed this question in considerable length, with particular reference to the *Anatomie*. I believe

that Sydenham's objection rested upon these grounds—that the data of microscopy can lead only to concepts that are remote, hypothetical, far removed from practical experience, and consequently too akin to speculation. Microscopic observations gave no indication of function and no firm practical knowledge. Sydenham, concerned only with the experiments of fruit, could perceive no fruit in microscopy.

The ideal of the virtuoso—knowledge for knowledge's sake—was contrary to Sydenham's principles. We see this viewpoint admirably portrayed in Sir Nicholas Gimrack, the parody of a virtuoso, who 'has studied these twenty years to find out the several sorts of spiders, and never cares for understanding mankind'. Sir Nicholas declared, "Tis below a virtuoso to trouble himself with men and manners. I study insects . . .' When he tried to learn to swim by observing the movements of a frog and then imitating them on a table, he declared, 'I content myself with the speculative part of swimming, I care not for the practick. I seldom bring anything to use . . .'[21] This, of course, was the direct opposite to Sydenham's viewpoint. And Sydenham lacked any faith that the study of spiders or the speculative aspects of swimming could eventually have any practical results. His scheme of values, based on faith and not on rational justification, differed completely from that of the virtuosos and the experimental scientists.

III

The empirical physician who only treated symptoms had no need to hunt for any sort of disease 'entity'. But the physician who would consider medicine a scientific discipline had to combine symptoms into patterns, with constant relationships. Some obvious patterns came immediately to mind, discriminated one from the other through certain quite manifest features.

Some symptoms might be so characteristic—like the eruption in smallpox—that they could serve to mark out a disease entity. In other conditions the temporal course, e.g., the succession of febrile attacks in tertian or quartan fevers, could clearly identify a disease. In other ailments like the so-called continued fevers, the fever itself was the main feature. In others the fever would be only a part of a more distinctive pattern, and the principal symptoms involved definite and

localized parts of the body, such as quinsy, attacking the throat; or pleurisy, the chest; or rheumatism, the joints; or dysentery, the bowels.

We can best study Sydenham's views of disease through his conception of fever and febrile diseases. His concepts were relatively vague. Fever had long been recognized as a preternatural heat, spreading from the heart over the entire body—to use the formulation of Fernel[22] [*febris est calor praeter naturam e corde in omne corpus effusus*]. Such increased heat might be the major manifestations of disease, as in the intermittent or continued fevers. It would then be 'primary'. Or it might be a smaller part of a total picture, dominated by some other clinical appearances. In such case it might be termed 'secondary'.

Some febrile diseases occurred in epidemic form, others only sporadically. Sydenham devoted particular attention to the epidemic fevers, constantly trying to find distinguishing features that would characterize and distinguish disease entities. He also tried to find causal factors which would *explain* his observation. And in his search for causes, he indulged in inferences that are well worth study.

Diseases such as plague or smallpox might rage violently in some years and kill large numbers of people, while in other years the mortality might be low. The reason for such a variation Sydenham attributed to atmospheric changes, to 'some unknown constitution of the atmosphere', rather than 'to some morbid cause accumulated in the human frame'.[23] That is, the cause of the epidemics was external to the body and entered into the body.

In his analysis he laid considerable stress on the concept of 'epidemic constitution'. His usage of this term, not altogether consistent, seemed to have several different components. One was the dependence of epidemic diseases on the atmosphere as their causative agent. Then, there was the transitory character of any particular constitution. It might last for several years, but would be succeeded by another. For the years 1661–1675 inclusive Sydenham distinguished five different constitutions, five 'peculiar dispositions of the atmosphere, and as many peculiar species of epidemics'.[24] Then, there was an interdependence of certain diseases among themselves, to form a sort of interlocking pattern. Under a given epidemic constitution one disease might predominate but some other disease might have a special relationship to it. For example, an epidemic of intermittent fever might be accompanied by plague or dysentery.[25] Certain essential fevers and certain other

acute diseases tended to go together in rough conjunction, and the association he attributed to the constitution.

Under any one constitution the different disease species all had a common factor—they were all referable to a common cause, namely, 'the peculiar atmospheric diathesis'.[26] But what *were* '. . . those particles which are mixed with the atmosphere, which war against health, *and which determined the epidemic constitution*'? [italics mine][27] Sydenham had no clear-cut answer and certainly not any answer derived from direct observation. All his statements were inferential. At particular times, he said, the air is 'stuffed full of particles which are hostile to the economy of the human body'. These particles might arise from vapours that exhale from the 'inward bowels of the earth', or they might result from atmospheric changes induced by conjunction of heavenly bodies.[28] But however the miasmata arose, when they were breathed in and mixed with the blood they engendered epidemic disease.

The whole concept of epidemic constitutions served several functions. It represented an over-all environmental element in disease, a broad factor that affected whole populations. In this sense it differed from the non-naturals which also involved environmental agencies (such as the air or the ingesta), but had a much more limited application. Diseases that affected scattered individuals might, perhaps, be attributable to disturbances in the non-naturals, but if a disease affected large segments of population, in epidemic form, then the 'constitution' was deemed a more appropriate explanation.

This term did not specify either the type or the origin of the pathogenic factor. The concept of 'miasma' was, of course, thoroughly familiar from Hippocratic days. The concept of harmful 'particles' was also quite familiar as an epidemiologic mechanism. That the agency might stem from some conjunction of heavenly bodies is listed as a bare possibility, without much support. However, the precise origin or the essential features of the disease agent remained unknown. Sydenham could only postulate its presence, and attribute its locus to the atmosphere.

We may reasonably query whether Sydenham realized that he was dealing in the hated hypotheses, that he was—let us say *inferring* or *speculating*—not *observing*. I hazard the opinion that he had no sense of speculation in his assertions. Miasmata or particles represented only a low degree of abstraction. So long as Sydenham remained relatively close to concrete experience, he probably was not troubled by any

doubts. We might say that Sydenham was not given to thinking in highly abstract terms, and consequently, when he did deal in low-level abstractions, he was not aware of the difference between inference and observation.

In summary, the idea of constitutions implied an orderly pattern. Epidemics had considerable degrees of specificity; they had various characteristic clinical features; they showed various conjoined phenomena and particular associated diseases. Furthermore, there was a succession of such patterns. To explain all this Sydenham offered the concept of 'constitutions', a term which indicated that many phenomena seemed to belong together, that the conjunction was significant, and that external environmental agents were important in explaining epidemic diseases.

IV

Much of Sydenham's theory revolved around epidemic fevers, but he also was keenly interested in fever as a mechanism through which a disease was eliminated. If some foreign substance had gained access to the blood, or some change of the humours required a correction, then fever was one means of eliminating what was foreign, achieving coction, and restoring the humours to their normal state. The febrile commotions might have either of two aims—to eliminate some 'heterogeneous' and 'antagonistic' material; or else to change the state of the blood into some other diathesis. This latter concept was his way of accounting for fevers where the agency was quite obscure. Thus, men might be striken with fever even in the absence of an epidemic, even though their blood was 'pure' and 'untainted'. In such instances he attributed the illness to some change in the 'non-naturals'. As a result the blood tried 'to endue itself with a new state and condition, adapted to the new state and condition of its food or air'.[29] Even in such cases the blood would eliminate something in order to restore itself to a 'laudable diathesis'.

Fever clinically was an increased heat. To explain it Sydenham invoked a physiological mechanism of 'commotion' of the blood. 'Commotion' means a severe agitation or turbulence of component parts, a strong 'activity'. To clarify this he had no direct evidence but could rely only on analogy. He used the term 'fermentation' or

'ebullition'. Fermentation was the familiar process taking place in the juices of fruit, grains and vegetables, while ebullition was a bubbling over, with gas production, so prominent in many cases of vigorous fermentation. Sydenham made perfectly clear that ebullition and fermentation were terms used figuratively only, merely for the sake of illustration.

Fever involved the animal spirits, an agent, purely inferential, deemed to be material, but the finest and lightest of materials. The animal spirits were related to the spirits involved in fermentation. For Sydenham fermentation and fever represented similar activities. In both, spirits were excited into greater 'activity' under appropriate circumstances.

He gave further metaphorical explanation in discussing the intermittent fevers which occurred in the spring time. Viscid humours, he claimed, had been accumulating in the blood over the winter, and against these viscid humours the animal spirits struggled. 'They try to fly off; but they get entangled and netted in, and held back. This it is that excites the vernal ebullitions. Exactly in this manner do full beer barrels that have lain long in cool cellars . . . begin to work as soon as they are set near a fire, when their liquor becomes ready to fly. The blood does the same.'[30] The idea of spirits 'working' indicates the readiness to undergo 'ebullition', and the term 'fly' [*avolare*, and *ad volatum pronus*] indicates the vigorous activity. The animal spirits in the blood, struggling against viscid matter, seemed to have an analogy with the fermentation of beer when exposed to heat.

Febrile commotion of the blood took place for a *purpose*. Fever was nature's 'instrument' to separate off the 'pure' from the 'tainted'. Sydenham recognized a febrile 'matter', and 'the concoction of the matter of fever simply means the separation of the sound from the unsound'.[31] If a foreign material were present, it must be eliminated; and the febrile ebullition—the increased action—of the blood was the means of accomplishing this.

Separation requires a 'suitable' degree of blood-activity, and the physician, when treating a patient, should aim at establishing or maintaining the proper degree. There were two major variables: one was the native power or activity of the blood, which would depend on such factors as age, constitution, and level of health; then, there was medication or other therapy, which might increase the febrile action if too low, diminish it if too high. Therapy could bring the native power of

the blood into the proper tempo, appropriate to the particular foreign agent to be eliminated, and suited to the character and level of the individual's natural activity. The physician must harmonize his therapeutics with the body's natural processes.

By extra effort, nature could compensate, so to speak, for a weakness. If we try to untangle what seems to be a devious train of thought,[32] we may interpret as follows: In epidemic diseases we find by observations a sort of 'standard' pattern. But in any series of diseased patients there will occur a considerable range of reactions. In many patients the disease would last longer than the norm, or the febrile reaction be less vigorous. Such cases Sydenham would explain by saying that the blood was 'too weak'. If the blood were too weak to eliminate the harmful agents through a 'normal' reaction, then nature would bring about a stronger reaction, such as more vigorous or more frequent paroxysms.

Sydenham was not content merely to note that some paroxysms were more intense in some patients than in others, or occurred more frequently. He had to render the phenomena rational. And he tried to do so in terms that were relatively close to experience.

Sydenham thought that the blood 'followed' the seasons. During the year, as winter progressed into spring, and spring in turn into summer, certain changes took place in the blood, which became 'proportionately exalted',[33] just as do the growing plants. The blood reached an 'acme', and then 'declined' as the season declined. Various unfavourable factors, such as blood loss or particular foods or environmental properties, could hasten the decline. In such a declining state the blood might be vulnerable to 'morbid impressions' from the 'particular constitutions of the atmosphere' epidemic at that time. And ebullitions might attack the humours of the blood that were degenerate, and then a fever arose.

Apparently the spirits in the blood varied according to the seasons; if fever attacked when the spirits were 'low', the ebullition was diminished and took a longer time to act. 'The blood is in a great measure stripped of its spirit; it is parched up by the preceding summer; it gets through with ebullition by slow movements and requires a long time for its despumation.'[34]

'Ebullition' and 'fermentation' referred by analogy to something taking place in the blood, as part of Nature's behaviour to separate morbific matter. But just what happened in the blood Sydenham did not say. Sometimes, when considering the pathogenesis of individual

diseases, he invoked a 'movement', that under normal conditions would be 'appropriate', but under other conditions either too weak or too strong. In smallpox, for example, an improper regime would excite the blood 'to a movement too violent for the proper separation of the variolous matter'.[35] He had neither the concepts nor the technique for analysing the activity but was satisfied to think of ebullition as some sort of vigorous motion. The motion constituted fever and was itself subject to various influences, both internal and environmental.

'Activity' of the blood is important in Sydenham's distinction between acute and chronic diseases. Primarily, of course, short-lasting diseases were acute, long-lasting ones chronic, but both types had a comparable mechanism. While the acute ailments were 'quickly brought to concoction', the chronic ones 'attained the same concoction, either very slowly or not at all'.[36] But there could be a substantial difference in pathogenesis.

Acute epidemic diseases were due to the 'hostile' particles in the air which, breathed in, mingled with the blood and resulted in disease. Nature then called in fever to help eliminate the hostile particles. Chronic diseases were not atmospheric in origin, but depended rather on 'indigestion of the humours', deriving from old age, or errors in the 'non-naturals', or impaired secretions. The humours, in disturbed balance, underwent 'fermentation' and 'putrefaction', and eventually 'break out'. Chronic disease thus resulted from 'depraved juices'. Their cause was the 'inability of Nature to concoct the humours'.[37] This inability might be irreversible.

V

To study Sydenham's methodology, we can contrast his analysis of acute and chronic diseases. For the former his discussion of plague offers considerable insight.

The pathogenesis of plague was obscure. The constitution of the atmosphere represented 'general' influences, and in addition he assumed a more specific 'miasma', some 'morbid principle' that derived mediately or immediately from the infected body. The exhalations from the corpses would propagate through the entire atmosphere until the atmosphere alone was sufficient to induce the infection. There was a

proviso, however, that the humours should be 'adapted to the receipt of the influence'.[38]

Sydenham thus distinguished between a fairly specific miasma which pervaded the atmosphere, and some unspecified general factors. A combination of factors was necessary—a disposition of the atmosphere, an infectious material, and in addition a condition of the humours appropriate to receive the infected material.

Epidemics did not consist merely of continuous propagations from one person to another, for if this were the case, once the plague had entered a city, deaths would succeed each other in a continuous series until no one was left. But this was contrary to observation, for epidemics ran a definite course, and gradually petered out. Furthermore, under apparently comparable conditions, one town might be severely affected, another remain wholly free. Consequently, Sydenham postulated some additional factor but had no inkling what it might be. His phraseology was, that plague cannot become epidemic 'except with the conditions of a favourable atmospheric diathesis'.[39] A statement such as this merely calls attention to observed fact, but without providing any explanation.

Sydenham admitted his inability to define the *essence* of the disease and the various properties resulting therefrom. Nature veils the 'essences' of things in 'deepest darkness'. The 'essence', in his usage, seems to be the 'cause' of the various properties, and this, for plague, was entirely unknown. But Sydenham, as an empiricist, indicated that 'how we can cure diseases, whilst we know nothing of their causes, gives us no trouble. It is not by the knowledge of causes, but by that of methods at once suitable and approved by experience, that the cure of the majority of diseases is accomplished.'[40]

This is a crucial passage for understanding 17th-century medical thought. It uses the term 'cause' in the particular sense of something hidden—the essence or quiddity which made a thing what it was, directed its course and development, and produced its properties and manifestations. But even if this essence were unknown, physicians could still treat the disease in empirical fashion by carefully observing the symptoms, and the effects of various medications.

The term 'cause' has many different usages and the failure to keep them distinct produces confusion. 'Cause' in the sense indicated above is quite different from an observable 'causal factor' as, for example, an error in diet, or exposure to heat or cold or dampness. Still different is

the usage that considered the 'action' of the blood as the 'cause' of the depumation or depuration that promoted recovery from a disease. Although he denied knowledge of causes in the sense of essence, Sydenham did discuss causation in the sense that we today would call pathogenesis.

He considered plague a special kind of fever which originated 'in the inflammation of the more spirituous particles of the blood'[41]—clearly implying that this inflammation was 'causal'. Nature tried to eliminate the inflamed particles by excretion, through 'emunctories', and the particles became pus, giving rise to tumours.

In the treatment of disease we can either follow nature and help her along the path that she has chosen; or else we can find some other method, 'safer than that of Nature's, and different from it'.[42] In regard to plague we are not able to do much to help nature. In true plague, nature produced abscesses and thus provided an outlet for the harmful matter. Therapy cannot promote abscesses successfully. In other diseases the physician should closely follow the conduct of nature, but in plague he must renounce her guidance. Therefore, the physician must find some other method, different from nature's. Sydenham indicated two forms of treatment, blood-letting and diaphoresis.

Blood-letting would control and diminish the ebullience of the blood. When the blood was too active or strong, blood-letting would reduce the strength and activity. Sydenham could find many cases, drawn from recorded literature, hearsay, and personal experience, indicating how beneficial was blood-letting in the plague, provided it was performed early enough and to a sufficient degree. He made no attempt to weigh circumstances of the different cases, to ensure that they were at all comparable, or to exercise any sort of controls. His attitude toward evidence was completely uncritical.

Sydenham considered diaphoresis even better than blood-letting to achieve the 'dissipation of the ferment of the plague'. If the patients did not sweat readily, stubborn attempts to induce diaphoresis might have bad effects and make the victim break out in plague-spots instead of perspiration.[43] On the other hand, any tendency to diaphoresis should not 'be checked too soon, i.e., before the whole of the morbific matter is thoroughly dispersed'.[44] Otherwise the matter which ought to get excreted in buboes, got 'withdrawn', and instead of developing into 'legitimate abscesses', struck inward with fatal effect.

He claimed remarkable success through his adoption of blood-letting,

but he complained that often he was prevented from using this treatment by 'the prejudices of the bystanders'—with unhappy results for the patient. It was important to take either no blood at all or else a sufficient quantity. Indeed, he narrated one case report[45] where the patient was much improved after blood-letting. 'Toward the close of the day I visited him, and found him much better; his friends being strongly opposed to a further loss of blood, in favour of which I strongly contended. I explained to them that one more bleeding alone was wanted, and that that would place him out of danger; that if this were opposed it would have been better for him not to have been bled at all . . . In one word, I said he must either be bled or die.' Alas! The patient died, for lack of one more blood-letting. Said Sydenham, by way of explanation, 'The remains of the peccant matter which ought to have been wholly eradicated, were retained in the blood, to the perversion of its whole mass, whilst their excessive subtilty broke up the tissues. The previous phlebotomy had precluded the evacuation in the way of abscesses.'

This type of reasoning had extraordinary similarity to that which Molière was so sharply satirizing. We are reminded, for example, of the scene in *L'Amour Médecin*, produced at almost precisely the same time. This comedy was first played in September, 1665. In it one physician declared of the patient, that she must be bled as soon as possible, while another physician emphatically denied it, but advised an emetic. Said the one, 'I maintain that an emetic will kill her', to which the other replied, 'And I, that bleeding will cause her to die.' Speaking to the father, one said, 'If you do not have your daughter bled at once, she will die'; and the other, 'If you do have her bled, she will not live more than fifteen minutes.'[46]

The death of his patient confirmed Sydenham in his belief that further phlebotomy would have saved his life. Molière, a layman, would have been highly sceptical. It is quite erroneous, I believe, to regard Sydenham as a sceptic. Instead, he was a strong doctrinaire, who bent the observations to fit his preconceived ideas, who lacked critical acumen towards his own ideas. Instead, he could always find evidence to prove what he wanted to prove. Sydenham had the same faults that he condemned in 'rationalists', with this exception, that he did concern himself more with observation and less with highly abstract concepts. But when he indulged in inference, as he constantly did, his results were just as uncritical. The significant feature is not that he failed

to critically scrutinize his inferences, but rather that he thought he was avoiding the speculation and hypothesis which he condemned.

On the other hand, I do not intend these remarks as any condemnation or derogation of Sydenham. He was one of the leading physicians of his day; he was an uncritical doctrinaire. Hence we must conclude that the leading physicians *were* doctrinaire, and furthermore that the labels of 'empiricist' and 'rationalist' are perhaps misleading. Study of Sydenham indicates that the teachings of Francis Bacon simply had not reached the medical profession at that time, despite abundant lip service.

We find a comparable methodology in Sydenham's views on chronic disease, as opposed to an acute one. Most illuminating is his dissertation on gout, published in 1683, somewhat later than the other writings we have so far considered. But his basic attitudes toward disease had not changed in the interval. Sydenham himself suffered severely from gout and his description of the symptoms has a vividness and accuracy that makes it a classic in the history of medicine. For our purposes, however, the symptomatology of the attacks lies somewhat to one side. I wish to take up his concepts of pathogenesis and their relation to his recommended therapy.

Gout was a chronic disease, in which was an 'accumulation of the inconcocted matter which . . . falls upon the joints'.[47] Whereas in acute diseases, especially epidemics, the causes are the particles in the atmosphere 'hostile to the economy of the human body', the chronic diseases were not atmospheric in origin. They were due to 'indigestion of the humours',[48]—what modern terminology might call a disorder of metabolism. For the humoralists, material entering the body underwent a transformation from a crude to a finished state. What could not be assimilated could be excreted. If the transformation went according to a normal pattern, the patient was healthy. But if the patient was not healthy, then coction, assimilation and excretion were not normal.

All this, however, is merely verbal juggling. Can the physician escape from tautology? Can he indicate concrete empirical data which can have real explanatory value and furnish more meaningful differentia? Sydenham's discussion of gout offers an interesting combination of explanation and tautology.

That gout was an 'impaired concoction' does not tell us much, but Sydenham indicated some of the specific pathogenetic—'causal'—factors. The patients are either old or have induced a premature old

age by excessive indulgence, particularly in food and drink and also in venery. The ferments appropriate for digestion have been weakened or annihilated, and the blood is overwhelmed by the dregs of humours,[49] and concoction is badly performed. The spirits are weakened and prostrated. But this alone is not adequate cause for gout, for otherwise, the victim of any long illness would be gouty, obviously not the case. More than one cause is operative. The gout attacks those who are hearty and robust, but only 'during the decline of their heat and natural spirits'.

Without in any way being systematic, Sydenham threw light on some problems of causation. He distinguished the *antecedent cause*, the underlying mechanisms responsible for the disease, from the *causa continens* [continuing cause], the actual material whose immediate presence represented the disease and whose elimination alleviated the disease. The distinction, of venerable lineage, was quite basic in 17th- and 18th-century thought. In gout the antecedent cause was 'the indigestion of the humours, arising from a defect of the spirits'.[50] But the *causa continens* was 'not only that which actually takes hold of the joints, and so forms a paroxysm, but also that which lurks in the blood, and is incompetent to the due separation'. [The Latin, *in sanguine delitescens separationi hactenus est inepta*, is, I believe, better translated, 'as yet unsuitable for separation'.] In present-day terms we can make a rough equivalent if we think of the *continuing cause* as uric acid, and the *antecedent cause* as the underlying disorder of uric acid metabolism.

Therapy revolves around causes. To eliminate the *causa continens*—the accumulated peccant and unconcocted material—there were three major remedies: bleeding, purging, and sweating. Bleeding was bad because the strength and vigour of the blood, already weakened, were inadequate to eliminate the peccant matter. Bleeding would weaken the 'force' of the blood still more. Purging was also bad, for in gout nature normally eliminated the peccant material on the joints, and a purge would change the mode of elimination. What had been meant for the joints would then be driven to the viscera, and endanger the patient's life.[51] [Sydenham did not clarify any possible mechanism for this translocation. It was pure hypothesis.] Nature would, by itself, 'separate the morbific matter and deposit it on the joints', and purging would only confuse the animal spirits—i.e., the active agent.[52]

Sweating, the third possible remedy, would divert the disease not

to the viscera but rather to the surface. Sweating, however, was bad because it would separate the humours while they were still crude and not yet ready for separation. It might force the peccant material upon the joints, and even, by over-stimulation, bring on an apoplexy.

Sydenham's reasoning seems strongly *ad hoc* to justify his preconceived ideas. He did not make clear why sweats might in one disease get rid of the actual peccant material and thus help the patient, and in another case eliminate only crude and unconcocted humours and actually harm the patient. Presumably he had first tried out various remedies empirically, and then sought theoretical justification why they helped in one case, not in another.

Interestingly enough, he was constantly attacking the empirics, who used particular remedies like purges, and claimed success. For Sydenham a rationale was essential. He believed in experience and observation, but he also wanted to justify and explain his observations. He required, so to speak, a *legitimacy* which only theoretical justification could furnish. Without this a practitioner was merely an empiric. And his attempts at such theoretical justification fell back on familiar humoral concepts, with overtones of primary qualities, such as the hot and cold. These metaphysical abstractions he could condemn, but he could not do without them. He merely masked their importance by giving more attention to empirical observation—a matter of relative stress.

Sydenham did not think highly of bleeding, purging, or sweating as remedies for gout. More important was attacking the root of the disease, i.e., the diminished ability to concoct. The physician must try to strengthen the concoction and to avoid whatever might throw a greater strain on the impaired bodily powers. This involved primarily a hygienic regime—moderation in food and drink; avoidance of fermented liquors or substances of a heating nature; use of appropriate exercise, especially horse-back riding which 'strengthens and invigorates the different digestions'[53]; administration of 'digestives' which strengthen the blood and improve digestive powers and promote assimilation.[54]

He also recommended particular medicines, holding to polypharmacy with a variety of drugs. He favoured complex prescriptions where each ingredient would take part in the cure. 'The more numerous the simples, the more potent the medicine . . . the mutual fermentation of the simples heightens their virtue, producing a *tertium quid*, of which the virtue, as a whole, is greater than the virtue of the sum of its ele-

ments.'[55] Constantly he insisted on rational indications for therapy, lest the physician act like an empiric.

In gout, however, the basic defect lay in the whole 'system', and no single remedy, briefly administered, could change it. 'The system must be transformed. The man **must** be made anew. . . . For years together a man has drunk and feasted—has omitted his usual exercise —has grown slow and sluggish—has been over-studious or over-anxious . . .' The various ferments had been perverted and the animal spirits, the primary instruments of concoction, oppressed. Hence the humours accumulated abnormally and broke forth. The bodily economy was turned thoroughly upside-down. The symptoms represented the method whereby nature drove forth the morbific matter.[56]

The therapy of gout must aim to change the whole habit of the body 'and restore it, as far as age and other circumstances will permit, to its original constitution'.[57] It was not enough to institute merely symptomatic treatment, because the manifestation of the disease represented nature's way of eliminating the abnormal matter, and alleviating symptoms did not alter the basic disorder. Merely checking the symptoms only rendered the true cure more difficult.

In his highly influential writings, Sydenham emphasized the importance of 'experience', but he also disapproved of the 'empiric' who had no rationale for his remedies. Hence Sydenham constantly sought justification for what he did. He constantly fell back on theory but in so doing he still remained relatively close to experience. His generalizations were relatively modest and he did not leap directly from immediate observation to abstract principles. Nevertheless, he accepted theories and presuppositions (as well as much empirical data) in a quite uncritical fashion, and much of his own inferential reasonings were markedly uncritical.

A critical attitude did eventually develop among physicians but the story of its emergence is the story of the whole 'medical enlightenment'. Sydenham represents one thread in a complex pattern. When we study this pattern we realize that 'experience' and 'reason' are reciprocal and interdependent, reflecting attitudes that themselves mirror an entire culture. The history of medicine, with its intricate counterpoint of 'theories' and 'observations', is intimately bound up with cultural movements.

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Chapter Four

Shifting Patterns

THE previous chapters have discussed a few major figures whose doctrines, differing markedly one from the other, moulded the theory and practice of medicine in the latter 17th century. But medical history involves far more than particular doctrines of great leaders. How did the teachings of these great leaders get transformed into actual practice? How did they react with the intellectual currents of the times? What facilitated change and what were the factors of resistance? How were new ideas regarded not only by medical men but by other members of the community? In this chapter I wish to consider a number of figures for the light they throw on these questions. I consider them not for their substantive teachings but rather for their attitudes and points of view.

The transformation to modern medicine depended on new doctrines and concepts, but perhaps even more on new attitudes of thought. Medical enlightenment required new values, new standards of validity, new critical acumen. We find relevant discussions in the splendid text of Jones[1] and other fine secondary sources[2] dealing with the so-called 'Battle of the Ancients and Moderns'. Some of the details are quite significant for medical history, even though I cannot agree with all the interpretations.

The figures that I choose for discussion are quite diverse, ranging from Sir Kenelm Digby (1603–1665) to Robert Hooke (1635–1703) and showing widely differing degrees of critical methodology, of clear thinking, of prejudice, of gullibility, of faith. In the aggregate they show the shifting and uncertain patterns that ran through the intellectual life of the era.

SIR KENELM DIGBY

If we want to study the development of critical acumen in medicine, we find an extremely significant document in the work of Sir Kenelm Digby, *The Powder of Sympathy*. Sir Kenelm's place in 17th-century British science is secure. A recent full-length biography by Petersson[3] tells us a lot about the man and his period, but little attention has been paid to the type of reasoning he displayed in his *Powder of Sympathy* —an essentially 'medical' text. This work, originally a speech that Digby made in France in 1657, was published in French in 1658 and later translated into English. I have used the 1669 edition.[4]

Digby provided an interesting case report. A Mr. James Howel [Howell] suffered a severe hand wound. He had come upon two friends fighting a duel and, trying to intervene, he received severe cuts on both the palm and the back of the hand. The wound did not heal well, even though King James sent one of his own surgeons to dress it. Gangrene was threatened, and Howell approached Digby to get aid.

The date of this incident is somewhat uncertain. Digby's mention of James I places the event prior to 1625. Keynes' biography of Harvey makes note of the incident and quotes a letter of Howell, early in 1622, written prior to the wound.[5] Presumably the duel occurred between 1623 and 1625. Digby thus waited almost 35 years before discussing the therapy. During this interval the corpuscular philosophy had made great progress as an explanatory tool.

The case report really falls into two parts. There is the bare narration of the 'clinical' data; and then there is the expository and theoretical part, which explains how the particular therapy brought about the cure. When Digby used the powder of sympathy he was about 20 years of age. Presumably he acted purely empirically, unbothered by theoretical considerations. The later theoretical exposition indicates his interpretation of doctrines that had developed over the middle period of the century.

The clinical aspects are as follows: When Howell complained of threatened gangrene, Digby undertook to cure the wound without touching it. He asked for some object that had the patient's blood upon it. The original wound had been bound with a garter which, still bloody, was available. Digby dissolved some vitriol in water, and into

this solution he dipped the garter. The patient, some distance away, did not know what was taking place but suddenly declared that he felt better. Digby advised him to get rid of 'all your plasters; only keep the wound clean, and in a moderate temper 'twixt heat and cold.'[6] Later that day Digby removed the garter from the water and let it dry. 'It was scarce dry, but Mr. Howel's servant came running, to tell me that his Master felt as much burning as ever he had done, if not more.' Digby restored the garter into the solution, and the pain, he learned, promptly disappeared. And in five or six days the wounds were entirely healed.

How to explain this? The cure came about through the use of a powder—vitriol—which was not in any direct contact with the wound. There was some sort of action, but it was not direct. Instead, the activity took place by what was called 'sympathy', which, in this case, indicates 'action at a distance'. Digby gave a detailed exposition of the mode of action.

First he made an important distinction, that the healing power worked entirely through natural processes, not magic in any unfavourable sense. The early 17th century maintained a sharp distinction between two types of magic. One took place through the intervention of spirits, influenced by various spells, incantations, and other esoteric devices. 'Natural magic', however, represented the activity of nature alone, entirely impersonal, without any intervention of spirits—whether celestial or infernal, virtuous or evil. The impersonal rule of nature, involved in natural magic, represented a sharp contrast to superstition, sorcery, and witchcraft. Digby emphasized that what he propounded, however difficult to believe, represented purely natural phenomena. He expounded seven interdigitating principles, which in the aggregate provided a complete explanation of the cure.

The first principle concerned light, which he considered to be a material and corporeal substance that travelled with 'marvelous celerity' in straight lines. If the light met a hard body, it bounced back just as a ball bounces back from a wall. The second principle indicated that rays of light bouncing off from a surface carry with them some particle of the surface upon which they have just impinged, in the same way that a tennis ball, bouncing from a moist wall, will carry away some of the moisture. Digby gave an analogy, that a wet cloth, drying before the fire, reflects the 'fiery rays' which carry away some 'small moist bodies', making a mist between the cloth and the fire. Comparably, in the early

morning, the rays of the sun striking the earth which has been moistened by dew, raises a mist which gradually rarefies as the sun continues to draw it upward.[7] In other words, under the influence of light, atoms leave the surface of an object and pass into the air.

His third principle: the air is full of 'small bodies or atoms'.[8] Quite obviously the air contained many different kinds of particles. For example, there was moisture which produced the phenomenon we call deliquescence. Particles of scent revealed the existence of odorous substances. The smoke or soot in the air held corrosive particles which could blacken silver, produce ulceration in human lungs, and affect household goods. We do not see the particles themselves, but we know them through their effects.

To dispel any wonder that the air could hold so many particles of such diverse types, Digby propounded his fourth principle, that 'every body, be it never so little, is divisible in infinitum'.[9] Hence, bodies can give off extremely minute particles which can be readily carried great distances. He had no difficulty in indicating the passage of particles in the air, moved by the wind. And since particles could be infinitely small, they may 'easily be carried away . . . by the rays that file in a moment from the Sun to the Earth'.[10]

To explain the healing phenomenon—healing at a distance—Digby had to have some sort of specific directing agency to bring the medication and the patient into relationship. To do this he introduced certain further concepts. As his fifth major principle he indicated that the small bodies, which fill the air, can deviate from their normal course by various interferences. He mentioned several natural phenomena which could cause particles to take a course other than what they would have taken without intervention. Thus, certain kinds of atoms could be altered in their course by the phenomenon of magnetism, and similarly with static electricity. And—particularly important for his exposition—fire could cause a re-direction of particulate matter. Quite obviously fire could create or re-direct air currents which could carry particulate matter into courses that would not take place without the mediation of the fire.

But at this point Digby introduced, quite casually, a further conception quite current in Galenic authors. This involved the confusion between 'heat' in its thermic sense and 'heat' as a sign of activity. 'Heat' represented a quality existing in varying degrees. Fire atoms were deemed the lightest and most active. Conversely, whatever

appeared highly active must necessarily be fire. 'Spirits' represented the 'hottest' particles. Pigeons, cats, and dogs, were considered 'hot Animals, which have continually a great transpiration or evaporation of Spirits'.[11] Onions are 'hot bodies which perpetually exhale fiery parts, (as appears by the strength of their smel [*sic*]').[12] Thus, the term 'fire' or 'heat' he applied to those biological entities which have extremely minute size, high 'activity', and stimulating or 'sharp' properties. All these were inextricably mixed together in the concept of 'heat' or 'fire'.

Now Digby had already shown that fire can 'attract' air (i.e., it can alter the pressure relationships). Then he employed the metaphorical senses of 'fire' and assumed that the phenomena produced by visible (thermic) fire would also occur with metaphorical fire, deemed to exist in living things.

Once we firmly grasp this idea, we can appreciate his sixth principle, which is quite complex but which involves the concept which we would phrase, 'like attracts like'. However, he did not phrase it quite so simply. By many different examples he claimed to demonstrate that 'Bodies, which draw the atomes dispersed in the air, attract [to] themselves such as are of their own nature, with a greater force and energy, than other heterogeneous and strange atoms.'[13] This is the crux of his exposition, and represents the concept of 'sympathy'. He gave many and diverse examples, all bearing on this same principle. For example, a yawn may seem to 'travel' from one person to another; laughter may act as a contagion and sweep over a crowd; or a melancholic person may induce a sombre mood in another. For phenomena such as these the term 'sympathy' seems not inappropriate. Still another plausible example derived from music—the 'sympathetic vibration' which two taut strings may exhibit. A vibrating string can, entirely without contact, induce vibrations in another held some distance away. Such phenomena Digby would explain by the passage of atoms.

Digby adduced other instances of less plausible nature. For example, he declared that preserved venison, no matter how well prepared, becomes rank during the season of the year when the living deer are in heat. His explanation took this form—that the 'spirits' which come from the living animals are attracted by the dead flesh. Again, a tablecloth stained with red wine is easily cleaned in the season when the plants yielding the wine come naturally into flower. But at other times the spots do not get readily washed out. It is 'consentaneous to reason,

that every sort of wine attracts more willingly the Spirits of those Vines whence it comes, than of any other'.[14] This principle, generalized, is the essence of his doctrine, that atoms attract other atoms of their own nature with greater force and energy than they exhibit towards atoms which are heterogeneous. And this attraction of like to like can operate very powerfully, through vast distances, and over a long period of time.

The seventh and last principle brings us to the climax. When a body attracts to itself the atoms or spirits that are similar to itself, the attracting force also draws 'that which accompanies, and whatever sticks and is united to them'.[15] In other words, we have here a 'coupling' phenomenon, not dissimilar from what we see in immunology. For example, we know that antigens can be coupled to various substances, and when an antibody and an antigen unite, the material coupled to the antigen would automatically be involved in the reaction. However, it is merely a passenger, so to speak, and not an intrinsic part of the reaction. Digby's notion was not dissimilar. To phrase it differently, the attractive force which one body exerts upon minute particles in the air is also applicable to continuous substances which 'travel along' with the similar particles.

In summary, the powder of sympathy worked in this fashion: The garter, covered with the patient's blood, is steeped in a solution of vitriol that is kept in an open warm place. The combined action of the light and the gentle heat push the atoms of blood out into the surrounding air. However, atoms of vitriol accompany the blood atoms. Now the inflamed hand is 'hot'. It exhales abundant hot fiery spirits and (by analogy with the draughts created by hot air) draws to itself the surrounding air. This fiery exhalation from the hand, and the draught created thereby, creates a current of air which draws the blood atoms and the attached vitriol. On reaching the hand, the blood atoms, finding their original source 'whence they issued, will stay there, re-entering into their natural beds and primitive receptacles'. Since the atoms of blood are inseparably joined to the spirit of the vitriol, they will 'joyntly be imbibed together within all the corners, fibers, and orifices of the Veins which lye open about the wound; whence it must of necessity be refresht, and in fine imperceptibly cured'.[16] Thus, the healing agent is the vitriol, while the blood atoms merely act as an agency of transport.

Digby pointed to some of the complexities involved. Thus, he declared that exposing the vitriol to the sun might render its atoms

'more fitly disposed to be transported through the air',[17] and quite possibly the solar rays might have some direct effect. If the vitriol solution, with the blood-stained cloth, is placed into a closet where light and air cannot reach it, the wound does not receive any benefit from the powder. This would prove that the blood-plus-vitriol must have free access to air. And there would be similar loss of effectiveness if the basin and the blood-stained cloth were covered up. And again, if the cloth were soaked in lye before application of vitriol, no healing effect would take place. Presumably the blood would have been destroyed. On the other hand, a simple washing of the blood-stained cloth would not impair the remedy, since sufficient blood would remain on the cloth to promote activity.

Now Digby did not state that he actually performed these experiments. He merely stated that this is what happens, without presenting any evidence for his assertions. They were, we may suppose, 'experiments of reason', where the logical necessity of the theory indicated what would occur in fact. And so powerful was the reasoning that factual demonstration was not at all required. Digby did not distinguish between what 'might be' and what 'is'. A logical interrelationship was assumed to be the equivalent of existence.

Digby had engaged in a long chain of close reasoning, quite logical and founded on principles which he derived from contemporary physics. The whole explanation was highly rationalistic. It had, however, one fatal flaw—it lacked any concrete evidence that these alleged activities did *in fact* take place. Digby did not perceive any difference between 'It might be so' and 'It *is* so'.

MARCHMONT NEDHAM

Medically speaking, the new philosophy made rather chequered progress. Two distinct new influences were at work. One was the atomistic hypothesis in either its Cartesian or Gassendian form, but this, while of great theoretical importance, did not directly affect the practice of medicine (therapeutics). However, the second influence, derived from the chemists, particularly from Paracelsus,[18] directly concerned medical therapy. Chemists introduced new remedies into medicine, such as antimony and arsenic, whose use went hand in hand with appropriate

theory, and the primacy of the three 'elements', mercury, sulphur, and salt.

Proponents of chemical theory were highly critical of Galen, in both theory and practice. Whereas in France the conflict attained great bitterness, in England, as Debus has shown,[19] a greater spirit of compromise prevailed. And yet, although the conflict in England never reached the intensity witnessed on the Continent, there was still conflict enough, conflict that depended on factors largely social and economic, and only partly scientific or medical.

A quite rigid hierarchy enveloped medical practice, and the College of Physicians stood at the pinnacle. Its members were highly educated and the College enjoyed a monopoly which it tried to maintain and increase. The less-highly trained medical practitioners, the apothecaries and surgeons, were in constant conflict with the College, trying to break the monopoly and increase their own prestige, and well being. Clark's recent definitive study[20] describes the conflict in abundant and fascinating detail.

The medical chemists opposed the Establishment on several fronts. One was the natural hostility of the man-on-the-make towards the man-in-possession. The conservative is always under attack by those who would try to liberalize the rules and seek the privileges previously closely guarded. There were differences in philosophic attitude. The College and the conservatives tended towards what we may call the rationalistic viewpoint, while the chemists were empiricists in varying degree. In matters of doctrine, the chemists maintained a chemical philosophy and denied the conservative Galenic theory which, modified through the course of time, held the allegiance of the physicians. One facet of this struggle is brilliantly told by Thomas, in his description of 'The Society of Chemical Physitians'.[21]

Conservatives, the men in possession, suffer attack from those who would replace them, who would change the standards and the rules and call their demands the demand for freedom. At some periods the opposing forces meet in fairly straightforward collision. The 17th century, particularly the latter part, witnessed such a collision.

In regard to the chemical physicians, most scholars adopt a viewpoint that follows Jones,[22] a viewpoint with which I cannot agree. The neoterics have long since won out. They opposed the old Galenic schemes, and Galenism toppled. They opposed the old rationalism, and the old rationalism crumbled. They embraced an empirical viewpoint,

and empiricism triumphed. These propositions are indisputable, but we cannot conclude that therefore the individual neoterics were correct, that their methods were valid or their logic sound.

I refer particularly to writers such as Marchmont Nedham and Joseph Glanvill, whose places in medical history should, I believe, be severely re-evaluated. These men jumped on the bandwagon of the new science. In the history of science they stand as the 'good guys', while their opponents such as Twysden or Stubbe, who upheld the losing side, receive but little credit. Nedham I will take up first, as representing specifically the iatrochemical movement, while Glanvill has a more non-specific position in the quarrel between the ancients and the moderns.

Jones praised Nedham quite highly,[22] as 'fully cognizant of the importance of the scientific discoveries of the 16th and 17th centuries', and as one who would not be subservient to authority. He had, thought Jones, a 'faith in the observational and experimental method', derived from Francis Bacon. Nedham, according to Jones, gave 'Bacon's general rules specific application to medicine'. I cannot accept these evaluations without considerable modifications.

Nedham lived from 1620 to 1678.[23] His occupation is given as a journalist. Apparently, during the early part of his career, he 'also studied medicine, but soon discovered that his natural vocation was in journalism'. Certainly the period during which he lived had ample use for a vigorous and not too scrupulous writer. Nedham engaged in editing, newspaper writing, and pamphleteering, and was a strong opponent of Charles I. 'The scurrility and boldness of Nedham's writings soon made him notorious.' Several times he was apprehended and jailed, but released. In 1646 he was debarred from journalism and 'turned to medicine, and described himself on the title-page of a pamphlet published in 1647 as "Med. Pr." '. In 1647 he changed sides and joined the Royalist party, but later turned back to embrace the cause of the Commonwealth. He wrote against the restoration of the monarchy and in 1660 he fled from England and took refuge in Holland. However, some months later he obtained a pardon and was able to return to England. For the rest of his life, it is said, he lived by practising medicine, but he also engaged in pamphleteering. Indeed, the government of Charles II employed his pen to attack Parliamentary opposition.

We do not know details of his medical education, nor of the actual

amount of medical experience that he had. However, we must not derogate the man simply because of his background. This would be arguing *ad hominem*. Rather must we examine his claims on their own merits and judge the man's medical and scientific stature from his published medical writings.

The *Medela Medicinae* was published in 1665.[24] It is quite obviously a work of special pleading and not at all of original observation. Nedham occasionally referred to patients he himself had treated, but these references are few and play no significant part in his argument. The book is a congeries of quotations, assertions, arguments, inferences, refutations, and logic-chopping. I want to study the degree of logical and critical acumen and the rigour of his thinking.

Nedham, as a poorly qualified practitioner, aimed to gain special privileges and prestige for himself and those like himself, and to do so at the expense of the Establishment. Since the Establishment depended on tradition, the proper mode of attack was to overthrow tradition. Nedham tried to prove that the traditional Galenic modes were inadequate and unsatisfactory, and that progress would depend on the new chemical remedies.

There is no question that Nedham was an effective pamphleteer who could find many ways to denigrate his opponents. Thus, the College of Physicians was active against the chemists, the empirics, and the quacks, and this Nedham turned into a condemnation. Indeed, he said, '. . . even the common people came to see, that it was the Interest of the Collegiate Corporations of Physitians . . . not to permit a new laborious Sect of Philosophers, working Knowledg out of the Fire, by their Industry and Successes, to bring a reproach upon them for their Idleness'.[25] Undoubtedly, the College, by its natural but inappropriate search for personal gain, and its arch-conservatism, wrought much harm to its own cause.

He indulged in some interesting arguments that show the calibre of his mind. In pleading for the chemical medicines and the relatively untrained chemists or chemical practitioners, he brought out a rather complicated argument. He wanted to show that the Galenical treatments were of no value. To make his point he claimed that there was great alteration in the diseases 'now' from what they had been previously, and this he asserted as a fact of observation. 'He is but a dull Practiser that doth not yearly see Agues and Fevers appearing in new forms'.[26] He pointed to syphilis which seemed to be a new disease

appearing in the late 15th century and which, according even to 16th century authors, had changed its character within a relatively few years. He pointed out that syphilis became hereditary and could be transmitted by contact other than venereal. All this was manifest evidence that the disease had altered.

Scurvy, he indicated, was a disease relatively new, and quite as universal as syphilis and it too, he claimed, had changed its form very markedly. It no longer showed its ordinary external symptoms, but got 'dominion inwardly, plays most of its pranks in the more noble parts of the Body'. Scurvy, like syphilis, could get transmitted in a hereditary fashion. Syphilis and scurvy had joined forces to change the 'whole Frame of Nature in Mankind, and all the diseases thereto belonging'.[27]

Nedham then presented his evidence. Going through the motions of empirical examinations, he studied the '*Bils of Mortality*, which shew both the increase of the *Scurvy*, and the Malignity of it'.[28] In these death records the number of scurvy cases increased from 5, in 1630, to 103 in 1656, and since then the cases each year numbered between 70 and 90. This, he believed, proved that the disease was much altered and much more malignant than previously. Moreover, the recorded figures indicated only those who died with manifest symptoms of scurvy, whereas 'many hundreds' had died of diseases which 'in the main are Scorbutick; yet because they are not discernible by vulgar Physitians' are 'registred under the Name of other Diseases'.[29]

With comparable analysis Nedham indicated the increased mortality for other diseases. For rickets, the statistics gave 12 deaths in 1630 and 521 in 1660. This, he thought, showed that the disease was much changed in its nature, to a more destructive state. Consequently, as a conclusion, it is '*vain to attempt the curing of it with such Remedies and Rules as were used thirty or twenty years ago*' [italics mine]. Experience, he believed, shows that the older remedies will not 'reach' the disease now, and consequently men should be trying to find out new medicines, as the only means of diminishing 'the number of those little Innocents which are yearly snatch'd away by an untimely death'.[30] After considering several diseases he came to the conclusion that 'all manner of Diseases are alter'd from their old State and become almost wholly new'.[31]

The bad management to which the patients have been subjected provided another reason for the increase in these diseases. Nedham

criticized strongly the various cures that were in vogue, and the ignorant quacks who provided treatment. Those who treated the diseases lacked 'knowledge and prudence', and that was why the pox, instead of being cured, has only been 'palliated'. It got disguised, and the disease agent put on a different appearance, and in its disguised state passed for some other disease.[32]

There were different kinds of scurvy. One came from bad diet, another from inheritance through the parents, and another by contagion. These different varieties needed different kinds of cure. The scurvy which derived from heredity was incurable, he said, and consequently we should lay aside 'the old Notions and Maxims' and should introduce 'new Hypotheses and Speculations, and a laying of new Foundations (other than that of a narrow Colledge, and old Piles of Books) whereon to raise a Body of Physick fit for our Age and Climate'.[33] In this significant passage he indicated that the old therapy was without value; the old therapy was supported by the narrow College of Physicians and old authorities; and new ways of investigation were needed.

Pox and scurvy are so universal, he declared, that there is scarcely any disease 'which is not mystically complicated with one or both those Master-Maladies', which 'run universally through the loyns of the Sons and Daughters of Adam'.[34] Nedham piled up evidence to show that pox and scurvy were universal. Apart from direct contact and hereditary transmission, a further mode of spread he called 'accidental contagion', that is, through the intermediation of particles carried to a distance, designated as effluvia, or 'invisible atoms'. Nedham had absorbed some aspects of the corpuscular philosophy, and several times he referred approvingly of the teachings of Digby, regarding the passage of minute corpuscles (or 'bodikins') at considerable distances.[35] Moreover, the particles may lurk in the body without manifesting themselves until a considerably later time. He seized upon the contemporary view of smallpox and measles, whose cause was supposed to be transmitted from the mother's womb and to lie latent for many many years. In a similar way, he thought the 'pocky' or scorbutic ferments could lurk in the body.[36]

Nedham indicated additional ways by which the diseases could get disseminated and become universal. Besides sexual or other contact, and the mysterious ways of contagion at a distance, he emphasized the hereditary propagation of pox and scurvy. Diseases, he recognized, could pass from one generation to another but would sometimes skip

a generation and be transmitted from grandfather to grandson, without involving, say, the parent.[37]

An additional mode of propagation was through lactation, in the suckling of infants. That substances are transmitted from the nurse to the infant, by means of the milk, was well recognized. Nedham, without any critical examination, assumed that particles which could infect by contact or by heredity or by passage through the air, could enter the body of the nurse and thus pass to the infant. Quite obviously, everyone has the possibility of being infected with either the pox or the scurvy. And these conditions have altered all other diseases and thus induced a need for new remedies—which the chemists should supply.

The infant, if it escaped congenital infection, could acquire the disease from the nurse's milk. If this did not produce infection, the infant was ever after constantly exposed to infectious corpuscula or bodikins, either in the air, or by contact, whether sexual or non-sexual. And no one escaped. The disease was latent, or if it became manifest, did so under the guise of some other disease; or, at a minimum, altered any other disease to a new form. *Credo quia absurdum*.

By reason of their 'contagious quality' pox and scurvy have 'so insinuated themselves, that they are become universal, a part of our Humane Nature, and consequently inseparable from us, as well as from our Diseases'.[38] The two master diseases, in their combination with each other and with other diseases, produced new types of ailments or conditions with unusual and new symptoms. And just because the usual signs of the pox (or the scurvy) are lacking, does not mean that these diseases are not present, for they may disguise themselves, according to the bodily properties of those in whom the contagious matter alights.[39]

The reasoning is quite remarkable. Paraphrased, it goes like this: diseases have changed their character. Pox and scurvy are universal diseases; they may appear under their own nature or may simulate other diseases or, by participation, may complicate all other diseases. Since pox and scurvy are transmitted from parent to child (as well as from one sexual partner to the other) they are inescapable. Old remedies do not help, and consequently new remedies are necessary. The College of Physicians is the bastion of the old remedies, while the chemists are those who provide new remedies.

Nedham was greatly intrigued by the discoveries which the early microscopists had made, particularly the observations of minute animalculi too small to be seen by the naked eye. All of these Nedham

classified as 'worms' and placed in the same category as the more familiar parasitic worms that were grossly observable. With fairly characteristic reasoning he implicated all kinds of worms in the causation of disease. Thus, in an epidemic of fever, in 1660 and 1661, he noted that a great many of the patients had worms. Consequently, in treatment, he ordered juleps and other remedies that would kill worms. Then he continued to give such vermifuge medicines not only in fevers but in all other cases as well, and he noted that worms were frequently eliminated. Consequently the worms seemed to be an inescapable complication. No cure of the 'main disease' could be accomplished until the 'verminous humour or matter be extinguished or removed'. And consequently the worms he considered to be a 'concause' of the disease, if not the only cause. And this, even though he did not see any actual worms eliminated.[40]

In his chain of reasoning, the gaps are clear: Some patients with a distinct and recognizable disease—'fever'—had worms. Appropriate vermifuges eliminated the worms. *Therefore*, vermifuges are beneficial in all fevers, and in all disease, and no disease could be cured until the worms are eliminated. The worms are co-causes of all diseases, even though not actually visible. This type of reasoning reminds us of Hahnemann and his 'psora', the universal basis for all disease.[41]

'Worms' varied enormously. Nedham had already discussed the widespread agents of disease, the effluvia, atoms, corpuscles, bodykins, and ferments. Such agents, hitherto, he had categorized as inanimate. But in a later chapter he explicitly added agents that are animate and living creatures. Such worms, he indicated, are extremely fine, and 'insinuate themselves into clothing and other solid bodies, both porous and compact'. The plague, it was well recognized, was due to such 'animated effluviums'.[42] All sorts of putrid and decaying objects harboured these worms, clearly visible in a microscope. Such organisms can be seen by the microscope, and such visual observation, a matter of direct experience, he expected to be entirely convincing. Corruption went hand in hand with these microscopic worms. In addition, corruption could produce grossly visible animals, the familiar spontaneous generation.

Nedham indicated that bodies act on each other at a distance by giving off certain atoms, corpuscles, or particles, which are sometimes inanimate, and sometimes animate; that these particles have the power —ferments—to corrupt other bodies and convert the blood and

humours into their own bad nature.[43] In Hippocrates and Galen very little was said of worms except in fevers, but now worms are found in all sorts of diseases and, furthermore, said Nedham, these diseases vanish when the worms are eliminated by adding 'anti-verminous Medicines' to other remedies.[44] The 'invisible Vermin' if neglected, render most diseases incurable.[45]

Nedham then summed up this portion of his argument, entitling this chapter, 'The Insufficiency and Uselessness of the old way of Physick, in respect of Method and Medicines, arguing a necessity of a new.' He tried to achieve truth by reasoning, stating that if his premise is true, then the conclusion will certainly follow. He presented his argument in a conditional form. The protasis is complex: all manners of disease are more fatal than formerly; they are changed from their ancient state and condition; the causes of the change were not known to the ancient physicians; a new 'nature' has been introduced into both men and diseases [i.e., the pox and the scurvy]. For the conclusion or apodosis he indicated that the old 'Rules and Doctrines' for cure are no longer valid; that the old medicines must be useless; and that great encouragement should be given to those who try to establish new doctrines, new methods, and new rules of cure. Furthermore, his colleagues should apply themselves to finding new and more rational remedies.[46]

Nedham jumped on the bandwagon, and added his voice to those of real investigators, to plead for further inquiry into nature, further experiments, further research. He cast his vote with the new science and the new empiricism, the trends whose tide was slowly rising, but whose effectiveness still lay in the future. In brief, he joined those who claimed that the old—the ancient tradition still dominant in medical circles—did not work and that something new was needed. Further, he insisted that the new discoveries *did* work. And then, he put in his special pleading that the empirics should have liberty to practice.[47]

In the dispute between the ancients and the moderns, Nedham, as a proponent of the moderns, indicated what marvellous progress medicine had recently made. If we look back only 50 or 60 years, he said, we can see how poor medicine was, compared to what it is now, and in the preceding 20 years, no other science had made such advance in such a short time.[48]

We do not need to follow Nedham further, in his discussions of chemical principles, his expositions of van Helmont, Willis, and Boyle,

and his extensive destructive criticisms of Galenic and Hippocratic principles.

Nedham stands as a splendid example of correct conclusion and incorrect premises. The question then arises, how much credit does the man deserve? He has generalized hastily and falsely, on the basis of inadequate evidence, faulty reasoning, uncritical reliance on authorities, failure to consult experience. On the basis of bad logic, and neglect of scientific attitude, he nevertheless propounded 'conclusions' which the course of time has justified. His exhortations have a fine progressive ring, because he supported the winning side in the battle of the ancients and the moderns at a time when victory was still in doubt. But he himself exemplified none of the virtues of Bacon, Boyle, Willis, Hooke, or others who made genuine contributions.

Nedham's transgressions against logic, scholarship, or scientific method, brought a sharp reaction from his opponents. The reaction, I maintain, was far more logical and showed a vastly greater command of critical analytic thought than did Nedham's own arguments.

Nedham achieved two purposes. First as a pamphleteer and a controversialist, he publicized a point of view that ultimately triumphed. In the second place, by his very excesses he provoked critical analysis of his claims, and this promotion of critical acumen, in at least some of his opponents, helped to improve the standards of medical thinking and made the conservative physicians more critical of evidence. Medicine could not advance until its practitioners and investigators developed an incisive critical attitude, and Nedham—himself lacking in critical judgment—helped to bring about a critical attitude in his opponents.

JOHN TWYSDEN

Nedham's book initiated a substantial controversy, with several responses and counter-responses. The disputants and their books are discussed in Jones.[49] I have no wish to give a historical analysis of the conflict but only to indicate some thought-modes prevalent at the time. Consequently, I shall restrict myself to discussing one single work that exemplifies best the conservative viewpoint—the *Medicina Veterum vindicata: or An Answer To a Book, entituled Medela Medicinae*, by John Twysden, and published in 1666.[50]

Twysden is but little known in the history of medicine. Born in 1607, of an aristocratic family, he matriculated at Oxford but left without a degree. He studied law and was called to the bar in 1634. According to the *Dictionary of National Biography* he was in Paris in 1645, and in 1646 he received his M.D. degree at Angers. In 1654 he settled in London and became a candidate for the College of Physicians, to which he was elected as fellow in 1664. He had a strong interest in mathematics and published mathematical essays, but his medical publications include only the volume here discussed and a later book of the same kind.

The subtitle of the book indicates its contents: "In which the ancient Method and Rules are defended, and farther shewed, that there is no such change in the Diseases of this Age, or their Nature in general, that we should be obliged to an alteration of them.' The style of writing, unfortunately, is laboured, often obscure, and with the complications and convolutions that mark so much of 17th century prose. The over-all pattern is a chapter-by-chapter refutation of Nedham's book, but we fail to get any direct incisive orderly counter-attack.

Nevertheless, despite the many faults—and they are many—the book presents the conservative viewpoint in a remarkably commonsense fashion. Personal invective is by no means lacking, but is far less than in most of the 17th century pamphleteering. Twysden defended the traditional medicine, without any extreme doctrinairism. Most of the book seized on small points that Nedham had made, and tried to refute them. It would be rather unprofitable to follow these point by point, and I wish only to indicate some over-all attitudes.

Twysden was very aware of the conflict between ancients and moderns. In a passage which, although obscure, seems significant, he tried to lay down some points of methodology. He defended the basic doctrines of the ancients, such as the four elements, the doctrine of humours, and the combination of qualities. These were the 'principles upon which the Art is built', but he said that they 'are like those Eccentrick Circles and Epicycles in Astronomy, which are not necessarily true, yet serve to reconcile appearances, and therefore are of equal value, as if they were undoubtedly so'. By these concepts, he indicated, 'we are brought to a certain knowledge of that truth we seek for, at least such a knowledge as the nature of the thing sought for is capable of'.[51]

A passage such as this I would interpret as showing a mathematical

mind at work, applying a mathematical methodology to empirical medicine. *The basic concepts of ancient writers had heuristic value.* (This we can readily see today, if we read some of the supporters of the classical tradition, such as Fernel or Sennert, whose doctrines have been so criticized by the neoterics.) These doctrines, which he indicated might not really be true, nevertheless offered satisfactory support for the medical practice. And the medical practice based upon these doctrines he maintained to be quite satisfactory.

He objected strongly to Nedham's plea for 'liberty', a most ambiguous term. All men have the liberty to qualify themselves to join the former ranks of physicians. All have liberty to examine and judge the teachings of those who went before, and to accept them insofar as they are reasonable. All men should inquire after truth and new discoveries, a doctrine with which Twysden was in entire agreement. Twysden objected strongly, however, to the way that Nedham tried 'from his single authority and less experience, to overthrow all that hath been laid down before him'.[52] In a sense this represents the point of the entire rejoinder. Nedham had tried to overthrow the entire medical tradition, but his claims were not adequately supported. Twysden was clear-sighted enough to point out the faults in detail.

Nedham had made assertions without adequate evidence. For example, his opinion that scurvy-grass and watercress were not to be employed in scurvy, rested on his own authority only, 'a freak of his own brain'. The idea that the whole world was infected with the pox and with scurvy Twysden considered to be a 'conceit'. And similarly with the idea that all diseases were not only infectious by contact but also might transmit their poison over a distance.[53] These statements Twysden had recognized as generalizations way beyond any available evidence.

Much of the book took up relatively small specific points, as, that Nedham had misquoted or misinterpreted his authorities, or that quite different conclusions could be drawn from his quotations. More significantly, Twysden severely criticized the fundamental assertion of Nedham, that diseases of the 'present' time were of different nature from what they were formerly. Twysden used a scholastic type of refutation, forcing Nedham on the horns of a dilemma, and showing that either side was quite untenable. The dialectic activity, however, adds up only to this, that the assertions of Nedham simply ran contrary to straightforward observation. Twysden asked whether the various

diseases, the apoplexies and fevers, catarrhs and epilepsies, the diseases of ears, eyes, and teeth, that Hippocrates, Galen, and Avicenna treated, and now frequently affecting mankind—whether these diseases 'stand in need of new Definitions and new Curations from any alteration of their nature?'[54] In other words, he called on straightforward common-sense observation, to decide whether the ordinary diseases that made up the bulk of contemporary medical practice, and were also well known to the ancient writers, *had in fact changed their character.*

The new discoveries in anatomy and physiology, he declared, have not changed the nature of the diseases, nor have they altered the method of cure. The new discoveries, however much they might have revolutionized the concepts of medical theory, nevertheless did not have practical significance in the care of the patient.[55] We have seen that Boyle maintained the same point of view.

Nedham, while recommending new treatments, had not provided adequate evidence of their value. He condemned the old but did not replace it with a sound and reliable body of knowledge. But it is precisely this, according to Twysden, which was lacking, and the chemists had not adequately demonstrated that their methods were valuable. Indeed, no two chemical physicians could agree on what to do[56] and the various disagreements had selfish motives—each chemist trying to cry down the other, so that he can set himself up at the expense of his competitor.

There are thus two major points. First, that the new remedies did not have proven worth; and second, that the practice based on the chemical remedies was open to abuse by untrained and selfish empirics and quacks.

We can, of course, see a logical impasse. According to Nedham's view, as he himself had expounded it, we must not be content with the old remedies, but we must try new methods of cure. Such an exhortation rests on the intuition that the old modes of practice are less than optimally successful. On the other hand, Twysden maintained that we must not use new remedies until their worth has been proven, for otherwise the door lies open to all manner of abuse. The conservative will admit the need for change but insists that the change must have sound evidence before it can claim allegiance. The radical, discontented with the *status quo*, insists that only new methods can save the day.

Twysden took up in detail some of Nedham's claims and examined

them critically, and thereby gave us considerable insight into the critical acumen of the times. Much of the criticism is rather picayune, but I will try to indicate the important generalizations behind them. For example, the claim that new fevers appeared in the early 1660's that did not respond to the old remedies, brought this comment, that other practitioners 'far more judicious, have not made the like observations', and consequently perhaps Nedham has not used proper judgement. The ancient physicians, in their teachings, presupposed that therapeutic directions would be used by men of judgement and not 'Mountebanks'.[57]

Again, with considerable acumen, Twysden pointed out that 'ague' was a widely variable disease and no single case would exhibit all the signs described by all the different authors on the subject. We know, of course, that the 17th-century writers were confusing a number of different diseases under the same title. Twysden, although he did not know this, did realize that the manifestations of a single *name* could vary quite widely and would not necessarily admit a single uniform regimen.

Twysden indicated that the Bills of Mortality, on which Nedham had relied, were not adequate evidence, and that the names given to the causes of death did not really disclose the actual nature of the disease.[58] The data should not be used for unjustifiable inference.

The claim that lues and scurvy had become universal diseases could not bear critical examination. The transmission of pox by venereal contact was, of course, established; and so, too, the occasional dissemination by personal non-venereal contact, as by kissing. But the claims that pox (and scurvy) were spread by contagion at a distance (in contrast to direct contact) was not proven.[59]

The concept that the pox or the scurvy could take on the guise of some other disease drew particular criticism. Twysden, in essence, asked the annoying question, 'How do you know?' How could you possibly recognize a disease if it existed only 'in the garb of some other disease'? How can any disease 'that appears in its own garb' have any 'Scorbutical taint', unless 'there be also some Scorbutical colours appear'?[60] In those days there was no way to diagnose pox or scurvy except by physical examination, subjective complaints and history. There was no laboratory work to give a clue, no serological tests, no determination of vitamin C. Under these circumstances the query is entirely valid, how can you tell that a disease is present when, by

admission, there are no distinguishing signs which point to it? Nedham, of course, had committed a colossal begging the question—blandly assuming what needed to be proved.

Twysden readily conceded certain points to Nedham, agreeing that scurvy very commonly occurred in chronic diseases, and furthermore that lues was a very widespread disease. But, he went on, this is far from admitting that these diseases are so contagious that everyone must be infected without contact, or by 'I know not what Effluviums, invisible Atoms or Bodikins'.[61]

A further extremely acute criticism showed the true Baconian spirit. The discussion really concerned the nature of analogy and inferences. Twysden indicated, 'How fallacious that way of argumentation is, which is drawn from the application of Reason from one thing to another, contrary to Experience.' For example, granted that plague 'is infectious, and the Contagion passeth from one body to another, without any immediate contact; therefore every Disease that is infectious, as the Pox and Scurvy are, must do so too.'[62] Here we have a clear call for valid evidence, and a condemnation of hasty generalizations. Twysden, of course, was mistaken in thinking that scurvy was a contagious disease, but this error in no way affects his critical acumen.

Furthermore, Twysden, who supposedly represented the rationalist school, showed himself far more empirical than Nedham, and would not believe in the existence of a disease where he had no reason for so doing. He could not accept the claim that a patient might be infected with a disease of which there is no sign. Twysden indulged in his share of satire: 'Therefore if a man break his Shin, or cut his Finger' then Nedham should treat this patient for the pox.[63]

In a sort of summary Twysden indicated what he had done. He attacked the fallacies of his opponent's arguments, and showed that diseases had not altered in their nature; that the discovery of new remedies did not remove the virtue of those that have been previously used, 'but both may be good and stand together'. Man should search into nature, not to disparage the ancients, but to discover new truths. He indicated that for pox and scurvy—which were not necessarily new diseases, even though but relatively recently introduced into Europe—cures were available on the foundation of the old methods. In other words, their cure did not require an overthrow of old teachings. And further, even though diseases may be complicated, it was the function of the physician to use his judgement, and we must not expect any

single 'general Method' to apply equally to many different patients.[64] That is, he rejected any concept of universal remedy, but insisted that circumstances alter cases, that it is not reasonable to expect uniformity in diseases throughout their manifestations and course.

Twysden declared that he would commend any attempt to improve medicine, but we must be cautious not to assert an improvement where none exists. And he wrote, 'I do not yet find any of our new and learned speculations touching the motion of the Bloud, the carriage of the Chyle, the manner of sanguification, injection of Medicines into the veins, have proved hitherto very effectual.'[65] He emphasized he did not want to disparage in any way those who had found out these new discoveries, but he did emphasize his belief that they had not contributed to the cure of disease. And any claim that all ailments might be curable with one or two chemical preparations, used promiscuously, without consideration of the specific factors affecting the patient, can only be condemned.[66]

Twysden fought a losing fight, for chemistry did indeed revolutionize the theory and practice of medicine. But at the time that he wrote, these changes were far in the future. Twysden showed incomparably superior critical judgement, when compared with Nedham, and this, perhaps, is the most significant feature of the medical enlightenment—the growth in critical acumen rather than the accumulation of facts.

JOSEPH GLANVILL

Joseph Glanvill (1636–1680) represented a strange mixture of religious and scientific attitudes.[67] His formal education he obtained at Oxford where he graduated B.A., in 1655, and M.A., in 1658, following which he held several church appointments, eventually becoming chaplain in ordinary to Charles II, in 1672. At the same time he belonged to the Royal Society, to which he had been elected in December 1664. This membership, of course, merely indicated an interest in science and did not in any way imply a technical competence in the field.

The Royal Society, founded in 1660 and incorporated in 1662, supported the new science which involved two principal aspects—the corpuscular philosophy as its theory and the new inductive and experimental method as its practice. These new views directly contra-

vened the Aristotelian concepts and principles in science, and they also attacked by indirection the entire learning which derived from Aristotelianism—the doctrines pervasive in religion, education, and ethics, doctrines that permeated the everyday life of the educated and articulate man.

While the Royal Society concerned itself primarily with natural philosophy, the new ideas seemed to threaten various other aspects of cultural life, particularly in the sphere of religion. Indeed, the anti-Aristotelian attitudes generated a considerable amount of general resentment all along the conservative front, and this resentment tended to focus on the Royal Society. The Society, therefore, needed apologists and defenders, and in this capacity Glanvill served admirably.

Throughout his short life Glanvill manifested two distinct trends, on the one hand a marked credulity, the highpoint of which was, perhaps, his belief in witchcraft;[68] and on the other a critical attitude toward certain aspects of science. However, his critical acumen was concerned particularly with words rather than things. He had relatively little ability to deal with concrete events on a critical level, but he showed his talents in the criticism of concepts and ideas.

As a clergyman Glanvill naturally had much concern with problems of the soul and mind, the relations of mind or soul to the body, the relationship of the universe to God. At the same time he manifested a strong interest in the new mechanical philosophy and the new science. He was able to combine these interests in his writings. At the early age of 25 Glanvill wrote one of his most important works, *The Vanity of Dogmatising*, published in 1661. Slightly revised, it was republished in 1665 under the title, *Scepsis Scientifica*.[69] In it he presented interesting viewpoints on some significant problems, with much that was relevant to scientific methodology and medical doctrine.

The problem of the soul formed an indigestible lump which disturbed the course of both natural and speculative philosophy. Do soul and body represent two substances or one? If two, what is the relation between them? What is the relation of soul to mind, reason, and brain? What is the relation of soul to reason and to God? Descartes had but recently died and his sharply dualistic philosophy was enjoying great vogue. It allowed ready separation of science from religion. Science could study extended substance under a mathematico-mechanical guise, leaving mind and soul for the theologians. Bacon had already indicated the desirability of such separation.

Soul and body, once separated, cannot have a logically satisfying union. How, for example, can soul impart motion to the body? Glanvill did not offer any real solutions, but he did reflect some interesting points of view. He declared, 'That that should give motion to an unwieldy bulk, which it self hath neither bulk nor motion; is of as difficil an apprehension, as any mystery in nature.'[70] He did not deny the *fact* that soul influenced the body. He merely declared that he could not explain it.

The problem, of course, was complicated by the notion of finely material spirits, related to the nerves. These spirits directly moved the body—and the process was indeed remarkable. The paths that the spirits had to traverse were so complicated that '. . . it is wonderfull, that they should exactly perform their regular destinations without losing their way . . . neither can the wit of man tell how they are directed . . .'[71] How the soul could direct the spirits was perplexing. Spirits acting, e.g., on muscle by way of the nerves, might obey mechanical laws, but '. . . the first determination of spirits into such or such passages, is from the soul . . . [and] all the philosophy in the world cannot make it out to be purely Mechanicall.'[72]

Mechanical solutions, he held, could not explain the facts of life. Without guidance, matter could not become transformed into animal life. Without a principle of guidance we cannot explain '. . . why the Lilly does not exchange with the Daysie; or why it is not sometimes painted with the blush of the Rose.'[73] He felt that such data required a 'knowing agent'. Although ingenious attempts had been made to learn the methods of such operations, the explanations, he felt, could not withstand any 'severer examination'.[74] That is, he did not dogmatically close the door on mechanical explanation. He did not maintain the need for Substantial Forms as explanatory principles. Yet he *believed* that the mechanical explanation was not adequate to explain the facts. Mechanism, to be sure, had gone a great way in explaining behaviour. But in observing all aspects of nature, he had no faith that the mechanical philosophy would be able to provide sound, satisfying explanation.

Glanvill had no illusions regarding the difficulties of achieving truth. Knowledge lies deep, he declared. On the surface we have useless 'froth', while 'the pearl lies covered with a mass of water'.[75] Furthermore, truths are not isolated but hang together in a connected fashion where one part entails another. Observation and experiment can detect

probable error and we learn from the corrections that experience provides us. But—in a way echoing Bacon—sometimes experience cannot help us and we '. . . out-grow not those errors, which we have learn't before the alphabet'.[76] These views are obviously reminiscent of Bacon's Idols. Where we cannot correct our doctrines by experience, he seemed to say, we persist in our errors. And incidentally, his own belief in witchcraft represented a wry commentary on his doctrines.

Glanvill had important views on the nature of causality, that in many ways presaged the work of Hume.[77] We do not have any 'intuition' of the causal connexion between events. All we observe of a cause is the constancy of association. 'For the *causality* it self is *insensible* . . . to argue from a concomitancy to a causality, is not infallibly conclusive: Yea, in this way lies a notorious delusion.'[78] Causes intermingle. To find the '*just* and *true* proportions' is the task of science. And to believe that we have acquired knowledge from a 'few indistinct representations' is, he declared, 'a *flatulent vanity*'.[79] Furthermore, he was fully aware of the tentative nature of hypothesis and system. Hypothetical reasoning and system may produce a closed circle within which we are free from error, but it is all still hypothetical and not necessarily applicable to the world of nature.[80]

Discussions such as these make us realize that so-called natural philosophy could not be sharply separated off from the broader areas of metaphysics, logic, theology, and ethics. Glanvill illustrated the way in which an intelligent, well-educated Englishman, committed to the new science, tried to work out some of the problems that the new science entailed. He showed considerable critical acumen, but not enough. As commentary on this, we may note that Glanvill considered the sympathetic cure by weapon salve to be satisfactorily established, and he did, as mentioned above, believe in witchcraft. Obvious indeed is the gap between his principles and his practice. But this very gap forms an essential part of the history of ideas. It becomes especially significant when we note some aspects more specifically relevant to medicine.

In 1668 Glanvill published a little book entitled *Plus Ultra*,[81] whose sub-title indicated its scope: 'The Progress and Advancement of Knowledge Since the Days of Aristotle.' The manifest occasion for its appearance was a dispute which Glanvill had had with an older clergyman, Robert Crosse, who was violently attacking the Royal Society

as a conspiracy against the Church of England and who maintained the merits and virtues of the Aristotelians in contrast with the Moderns. Glanvill's book defended the Royal Society against charges of subversion, atheism, anti-clericalism, and all the accusations which ultra-conservatives had levelled against it. But it did not merely defend theological soundness. It tried to emphasize the positive aspects, the great benefits which the Royal Society was providing the country. This, in turn, necessitated a strong defence of the new science, whose promotion was the *raison d'être* of the Society.

In defending the Royal Society against charges of atheism, Glanvill indicated that science was not merely consistent with religion but even strongly supported the religious viewpoint. As he joined the Battle of the Ancients and the Moderns, he beat the drums for the Moderns, and while praising modern science condemned ancient medicine.

In his preface Glanvill apologized, in a way, for attacking Aristotle. He acknowledged the merits of much of Aristotle's work but objected to the disputatious aspects. The time spent on disputes could have been better spent in surveying the works of God—in other words, studying nature. The study of nature would promote religion and the glory of God. Glanvill emphasized the uselessness of verbal disputes. Knowledge derives from the study of things rather than from disputes about abstractions. Unfortunately, he was an enthusiast more at home with abstractions than with things, and his theological training had fitted him more for disputes than for observation and empirical judgement. He made wild claims and inadequately supported assertions, and yet he could make them all sound quite plausible. His one great merit, apparent to our eyes if not to his own contemporaries, was his support of the eventually winning side. The 'modern' science did triumph over the Aristotelian notions; mankind did derive benefit from studying the book of nature; many of the seemingly fantastic predictions did come true—or approximately so. And because he backed the winning side we tend to overlook his frequent bad logic and uncritical attitude. And when his contemporary, Henry Stubbe, pointed out the errors and uncritical exuberance, subsequent critics have condemned Stubbe as reactionary.

Glanvill made some utterly indefensible statements about medicine. For example, the old philosophers, although 'their Wits were excellent'[82], did not elicit much practical advantage. Their knowledge he characterized as 'notion and dispute, which still runs round in a laby-

rinth of talk, but advances nothing'. Glanvill criticized the 'unfruitfulness of those methods of science, which in so many centuries never brought the world so much practical, beneficial knowledge, as would help towards the cure of a cut finger'.[83] This, he felt, was a 'palpable argument, that they [the ancient methods] were fundamental mistakes, and that the way was not right'.

Glanvill continued his polemics, 'And if the moderns can not shew more of the works of their philosophy in six years, than the Aristotelians can produce of theirs in more than thrice so many hundred, let them be loaded with all that contempt which is usually the reward of vain and unprofitable projectors.'[84] Such statements, of course, would obviously infuriate physicians who had extensive experience in the cure of human ailments. And particularly infuriating was the flimsy basis on which Glanvill erected his assertions.

When Glanvill published his remarks he had no supporting medical evidence. He praised the merits of the chemists, who were trying to introduce new remedies, but the benefits of the new chemistry were not really obvious. Indeed, the contrary seemed to obtain. Glanvill praised the new studies in anatomy, which had disclosed so many new structures unsuspected by the ancients. But in no way could he show any practical advantage from these discoveries. And the process of transfusion which occupied so much time in the Royal Society *Transactions* and meetings, was quite without proven value.

Glanvill quite misunderstood the Baconian distinction between 'experiments of light' and 'experiments of fruit'. He was praising the method of the new science, a method for which Boyle was the outstanding exemplar. The experimental method, which studied nature rather than disputed about terms, did indeed produce fruit. But this fruit took the form of new facts, opened up many avenues for the discovery of further facts, and helped to establish new theory. All this is quite different from the 'fruit' in the sense of immediate concrete practical advantage to mankind. Boyle's discovery in pneumatics or Hooke's microscopic observations, however interesting, did not make the lot of a common man any more comfortable, did not promote medical practice, did not, indeed, help in the slightest degree toward the cure of a cut finger. Shadwell's brilliant satire on *The Virtuoso*[85] indicates quite clearly the popular attitude toward the amateur scientist.

Glanvill was eager to support the new, but in his desire to denigrate the Aristotelians he flew in the face of manifest and obvious fact. No

amount of enthusiastic shouting could change the stubborn fact that in 1668 concrete practical advantages in medicine had *not* accrued from the workings of the Royal Society.

Glanvill did have a form of insight which we might best characterize as poetical. He did grasp the essence of what we like to call the 'scientific method', and he appreciated its significance. We can only regret that he did not apply it. He truly grasped the message that Bacon was trying to bring and in vigorous and effective prose he helped to popularize it. He realized that the Royal Society represented a bulwark of the new ideas and should be encouraged, since only in this way could the ideas themselves get propagated. He knew that the Royal Society '. . . aims at the benefit of universal mankind. For could they [unfriendly critics of the Society] expect that such mighty projects as these should ripen in a moment? Can a cedar shoot up out of the earth like a blade of grass? or an elephant grow to the vastness of his bulk, as soon as a little insect can be form'd of a drop of dew?'[86] Progress must 'proceed slowly, by degrees almost insensible'. The task is to 'overcome prejudices'. There is better employment for men 'than tying knots in bulrushes', and a well-built house is better than a castle in the air. 'We must seek and gather, observe and examine, and lay up in bank for the ages that come after. This is the business of the experimental philosophers'. No critic should expect everything to be done all at once. In accordance with this viewpoint, Glanvill's predictions what the future might bring have some real significance. We can only regret that the entire book did not proceed in this vein.

Concluding his work he presented an excellent picture of the ideals of philosophy. It taught men to beware of the 'infirmities of humane intellect', and rendered man 'wary and modest, diffident of the certainty of their conceptions'.[87] He drew a pleasant picture of the critical investigator, ready to learn and to detect his own mistakes, who can reason without passion, and suspend judgement where he does not clearly understand. These many splendid statements Glanvill himself honoured in the breach rather than the observance. In point of fact, he was much more concerned with polemics than with truth.

A physician whose way of life had been intimately bound up with a respected tradition, who had practised the teachings of this tradition, would have a predictable reaction to Glanvill's book. Stubbe was such a physician, and he paid attention to the numerous errors and unsupported claims which, to a physician, were not only irritating but

nothing short of ridiculous. Such a physician might, indeed, have sympathy with the new experimental method but Glanvill's argument was unfortunately all of a piece, involving Aristotelian philosophy, Galenic medicine, scholastic disputations, traditional learning, all in one mass of uniform condemnation. If Glanvill erected a single block-like argument that must stand or fall as a whole, then the palpable absurdities of some particular aspects would serve to condemn the whole.

HENRY STUBBE

Stubbe has not really had a fair trial before the learned world. He bitterly opposed the Royal Society and its spokesmen, Glanvill and Sprat.[88] To the modern mind Glanvill, Sprat, and the Royal Society represented progress. Stubbe, who opposed them, represented the losing side, and consequently takes on the role of anti-progress. Nevertheless, Stubbe was basically right in certain of his contentions. Glanvill had made some insupportable statements and Stubbe was not slow in condemning them.

Henry Stubbe (or Stubbs) had a rather chequered career. Born in 1632, he was educated at Oxford, receiving his B.A. in 1653 and his M.A. in 1656, with part of the intervening time spent in the parliamentary army. At the University he was noted for his classical learning, and he engaged in controversial writing against the universities and the clergy. He got into some difficulties because of this, and then in 1659 and 1660 began to practice medicine 'which had been his study for some years'.[89] Apparently he did quite well as a physician and enjoyed an extensive practice. Since he practised outside of London he did not come under the jurisdiction of the Royal College of Physicians. He wrote extensively, principally on non-medical subjects, and was given to polemics. His controversy with Glanvill is only one of many. In 1676 he died from accidental drowning and, interestingly enough, Joseph Glanvill preached the funeral oration.

In his dispute with Glanvill, Stubbe pitched his argument on a pragmatic basis. For him the important criterion was, What worked? In medicine, certainly, to cure the patient was more important than to hold a particular set of doctrines. In any conflict of values, where we must weigh opposing theories and opposing practices, the victory, he

said, must go to the side with the most effective practice. This point of view recurred again and again, in different guises, throughout Stubbe's book.

In general the writings are exceedingly dull reading. Stubbe tried to show Glanvill as an ignoramus, who talked boldly without really knowing what he was talking about. There is a great deal of what we today call 'nit-picking', where the disputants quoted authority, denied the validity of the opponent's authority, and made fine-spun arguments. Relying to some extent on debater's tricks, Stubbe showed the carelessness of many statements that the virtuosi had made, and then implied that little reliance could be placed on their other statements.

Stubbe resented exceedingly the fulsome and unwarranted praise which Glanvill bestowed on the Society, and the rash claims for priority. Stubbe was more concerned with trimming the Royal Society down to size than with mere blind defence of the ancients. And certainly, he made the point that even though members of the Royal Society had achieved much, their discoveries had often been anticipated by much earlier workers, or by those not belonging to the Society.

Despite the pervading dullness, some of the arguments illustrate quite well the medical temper and the relationship to what we call scientific method. Stubbe[90] indicated that various theoretical concepts such as the 'principles of DesCartes, and the figured atoms of Gassendus' were just as 'precarious as those of Aristotle, and less subjected to sense [i.e., perception]', and similarly with the 'Chymical Principles'. However, the notion that one set of concepts, even if abstract and quite removed from sense observation, might nevertheless have greater explanatory value than another hypothesis, he did not perceive.

In the latter 17th century a major dispute attended the use of chemical medicines. As we have seen in a previous section, the iatrochemists came into sharp conflict with the Galenic teachings. Stubbe tried to show that the ancient writers were by no means ignorant of chemistry, and that they had anticipated some of the more modern teaching and attitudes. But more important was the pragmatic test, that the chemical principles simply did not furnish a satisfactory explanation of disease,[91] nor did the chemical remedies in practice prove demonstrably superior to the Galenicals. Said Stubbe, 'I am confident, that whosoever shall enquire into the ill consequences of the two Pharmaceutics, will say, that if the Galenical be not always the most efficacious, it is always the most safe and innocent . . .'[92] The chemical remedies were often highly

destructive and dangerous, often unpredictable. Stubbe admitted that often the chemical medicines yielded good success,[93] but this need not necessarily be attributed to the virtues of chemistry, for '. . . as great cures are done by Countrey-physicians and Countrey-gentlewomen oftentimes, as any ever were wrought by Chymistry'.[94] He failed to note that this objection applied equally well to successes from Galenic remedies.

The general philosophy of Stubbe is admirable: 'It is impossible for any person to be a great physician, who ties up himself to one sort of natural philosophy, as if it were really true and certain; the operation of medicaments is oftentimes such as answers not the principles of any philosophy, and the digestions of Nature are so different from those of Chymistry, her fires, her solvents, her filters, her furnaces and vessels, her mixtures and degrees of heat so discrepant, that there is no arguing from the one to the other; nor is there anything to be relied upon in Physick, but an exact knowledge of medicinal Phisiology (founded upon observation, not principles), semiotics, method of curing, and tried (not excogitated, not commanding) medicines . . .'[95] The physician should avoid strict reliance on theoretical principles, and he should not derive his treatment from these principles but rather from observation. The workings of nature are different from workings of experiments, or, in our present-day idiom, *in vivo* is quite different from *in vitro*.

The 16th and 17th centuries had made great progress in anatomy and this progress formed one of the stronger weapons that the Moderns wielded against the Ancients. Stubbe supported the honour of the Ancients. Admitting that Galen made many errors, Stubbe declared, 'I know not one that incommodates man *in point of practice* . . .'[96] The few defects seemed of no real significance, and Stubbe condemned the Moderns who stressed the few errors and passed over the great mass of all that was good. And he pointed out truly that the discovery of the circulation did not really have very great utility. 'It was thought at first that this circulation of the Blood would overthrow all the usual Methods of Physick and introduce new and beneficial discoveries in that part of Medicine which is Therapeutick.'[97] But the concept of the circulation did not change the usual methods of treatment, nor introduce any discoveries into therapeutics.

The transfusion of blood and the intravenous injection of medicine formed a major aspect of Royal Society activities at this time, and a

large part of the *Transactions* for the first ten years concerned experiments in these areas. Stubbe was highly critical. Apart from many picayune criticisms that deflated Glanvill's overweening claims, Stubbe's objections to transfusions rested on some important points. For one thing, there was no clear rationale. What did the transfusions try to do? Rectify the mass of disordered blood by supplying a few ounces of some other blood? 'They may as soon rectifie as much vinegar, or decayed wine, with the like proportion of good wine.'[98] Further, the procedure of transfusing animal blood into a human was physiologically faulty, since Stubbe realized—although he expressed it imperfectly—that in animals the metabolism and transformation of food into blood was different from what it was in humans. Animal digestion was not the same as human digestion and hence animal transfusion introduced foreign substances into the human body.

The blood of animals, he indicated, differed from that of humans, although the differences, not apparent to ordinary observation, required special procedures for their demonstration. Moreover, differences between one human blood and another could be identified by suitable examinations. Stubbe described a large number of experiments which we need not discuss here in detail. He attempted to arrive at some sort of discrimination, and concerned himself largely with colour-changes that occurred when blood was mixed with various chemical substances and solutions, such as alum, 'spirit of harts-horn', brandy, saltpetre solution, and the like. Colour change was one of the few end-results that he could observe with the technique available, and he assumed that these colour differences had significance for his argument. His results were of no value, and his methods riddled with flaws, but his intent, at least, had merit.

He concluded that these experiments of mixing blood with various chemical substances could reveal differences which mere exposure to heat could not show. Chemistry, at that time, was largely the process of analysing by fire, with varying degrees of heat, and then studying the resulting product. Stubbe claimed that his chemical experiments were more discriminating than tests by incineration. He thought he could show by his tests that different animal bloods, mixed with the same fluid, reacted differently. Hence, the blood of animals was not the same as that of man, and transfusion was 'rash and unsafe'.[99] Furthermore, by using the same 'analytical' procedures, he could show that the blood of a given person might differ from one time to another.

'There is a continual variation in the blood upon every disease, and often without it, during a state of health: that the blood of individuals of the same kinde [species] differs not only from it self, but in each other individual . . .'[100]

By modern standards the experiments show no scientific rigour. Stubbe had no appreciation of his uncontrolled variables (especially when he allowed the mixtures of blood to stand for a day or two, with attendant 'corruption'). While he had a vestige of quantitative approach, in that the relative amounts were approximately measured, the uncontrolled and unmeasured variables were so numerous that they rendered the experiment quite worthless. Thus, Stubbe used invalid evidence to draw conclusions that eventually turned out to be approximately correct. A strange similarity to Glanvill!

Stubbe concluded that the use of transfusions had no justification. 'Shall a man transfuse he knows not what, to correct he knows not what, God know how?' Transfusion was appropriate only for 'the worst and most irrational Empirics'[101]—the worst condemnation for any one who had intellectual pretentions.

The reader will note an inconsistency in Stubbe. On the one hand he defended any practice of medicine which *worked*, regardless whether the theory were faulty. On the other hand, he condemned transfusion because its theory had no rationale. Transfusion, as then practised, rested upon a false basis, and he indicated that it should not be allowed until justified by successful trials. But trials were not justified because the procedure was contrary to sound reasoning. This is a circular argument. The circle would have been cut across had the various trials been successful, but they were not successful. Transfusion did not work. Stubbe could criticize transfusion as faulty in both practice *and* theory.

In his conclusions Stubbe declared, ' 'Tis most prudential to insist upon experienced methods in Physick' [in other words, the Galenic practice], and he specifically condemned the 'phansies about spirits, salts, sulphur, fermentative fires in the heart, occasioned by heterogeneous mixtures, and the explication of the operations of medicaments by the new philosophers, either chymical or others'. These 'are all in vain, and cannot be allowed as a ground of practice, til justified by successful trials . . .'[102]

In the final sentences of the main presentation he made some important declarations which, however, have received virtually no attention. He insisted upon the difference between *in vivo* and *in vitro*, and

pointed out that when the blood is '*sub dominio animae*' [i.e., in the living body], then 'effects upon it are different from what when it is separate'. It is 'not conceivable that the fabrick of our bodies is purely mechanical: where the liquors would have the same effect on the blood in the body, and without: which they have not.'[103]

We need not go into details of Stubbe's other controversial writings against Glanvill. Jones, particularly in his *Ancients and Moderns*, has given the most detailed account, while important discussions are also found in Stimson,[104] Cope,[105] and Nicolson[106]. In general, sympathy in the dispute rested with Glanvill and although some kind words are allowed to flutter down upon Stubbe, he is generally characterized rather harshly by these authors. But so far as the medical aspects are concerned, my own interpretation is far more favourable to Stubbe, and far more critical of Glanvill.

Stubbe was probably not a very amiable person; he may have had unworthy motives in much that he did; and he comes down to us as a polemicist, and not in any sense a scientist or a philosopher. But he was a successful practising physician and maintained a physician's viewpoint. He resisted the uncritical enthusiasm of the neoterics and he provided valid objections to rash and shallow judgements. By providing critical acumen he made a substantial contribution to the growth of medical science.

SOME ITALIAN REACTIONARIES

In the quarrel between the Ancients and the Moderns, that part which centred around the Royal Society has received particular attention. But recently Adelmann, in his monumental study of Malpighi,[107] has brought together and made available a vast amount of data that derives from Italian sources. Malpighi represented the 'modern' scientist who was not, however, especially concerned with the mechanical philosophy or corpuscular doctrines, but rather with biological studies. As a pioneer in the newer progress, he aroused great enmity among the traditionalists and representatives of the old Establishment. Adelmann has presented us with considerable information dealing with the reactionary viewpoints, not, by any means, all directed toward Malpighi alone, but toward any of the innovators. Fisch[108] also has told us much

about the over-all struggle between the Ancients and the Moderns, in the 17th century.

Galenism was strong. In 1663, in Naples, the reactionaries indicated that chemical medicines were both unreliable and dangerous, and 'are not to be permitted without the approval of the Galenic physician'.[109] In 1665, one Lipari attacked the teachings of the neoterics, especially in regard to the circulation of the blood. Among the theses that he supported were, that it is rash to practise medicine without astrology, or to phlebotomize at the wrong aspect of the moon.[110] This book, which supported Galenic doctrines and claimed victories over the neoteric physicians, was largely directed against Malpighi.[111]

In 1678, Paolo Mini attacked Malpighi and all the progressive anatomists of that time. The students should abandon anatomy, 'that is, dissection, as a useless thing, performed only by persons of little talent and little brain, since the study of Galen's book *De Usu Partum* was alone sufficient'.[112] Mini claimed that knowledge gained by dissection and the microscope was useless to the physician, and while the neoterics had learned a lot about structure, they had failed to find out much about function. He claimed that chemistry would tell us how nature operated, and that the proper method of approach was chemical analysis and not the knife or the microscope. While this appeared at first to be a progressive view, actually Mini's system revolved around a Galenic framework, he kept to the Galenic faculties and held them to be responsible for the chemical reactions. He tried to save Galenic physiology at all costs.[113]

Adelmann has analysed this and various other disagreements[114] at considerable length, as part of his exposition of 17th-century Italian medicine. His study of Malpighi represents an indispensable source for all serious students of the period. A comparative view of the development of neoteric medicine in different countries would be quite illuminating but would lie outside my present purpose.

ROBERT HOOKE

At this point we may profitably review the ideals of scientific investigation as expressed in the 17th century. Robert Hooke (1635–1703), the curator of experiments for the Royal Society, published in 1665

his famous *Micrographia*,[115] one of the great contributions to science. Its particular importance for our purpose rests in the views which Hooke advanced in the dedication and the preface. With unrivalled clarity he described the methodology, derived largely from Bacon, that reflected the official view of the Royal Society.

In the preface[116] Hooke analysed his concepts of the proper scientific method. In discussing this preface, Jones[117] has stressed the emphasis which Hooke laid upon the practical workings of science. However, I wish to stress the theoretical implications which have particular relevance for medical problems.

Our scientific activities depend on the 'operations of the sense, memory, and reason'. The senses of man are quite imperfect and our memories quite faulty. Consequently, the functions of reason—arguing, concluding, defining, judging—are all quite uncertain and imperfect. Our errors, he declared, 'proceed either from the narrowness and wandering of our senses, from the slipperiness or delusion of our memory, from the confinement or rashness of our understanding . . .' The remedies lie in the experimental philosophy.

First of all, we must correct the errors of sense. In any scientific investigation we must first demand, What are the facts? 'There should be a scrupulous choice, and a strict examination, of the reality, constancy, and certainty of the particulars that we admit.' And these 'particulars' must be evaluated not merely by 'number', but also by 'weight'. That is, some are more 'instructive' than others and nature seems often 'to make many doublings and turnings, and to use some kind of art in indeavouring to avoid our discovery'.

Since the senses are highly imperfect, we must extend their range and accuracy through appropriate instruments. Through the use of such tools as microscopes and telescopes we can peer into the 'secret workings of nature'.

Hooke continued the Baconian error, that it is not too difficult to reform philosophy, requiring not so much '. . . strength of imagination, or exactness of method, or depth of contemplation . . . as a sincere hand, and a faithful eye to examine, and to record, the things themselves as they appear'. This naive attitude had at least the merit of stressing observation and discouraging speculation. Too long had science been 'only a work of the brain and the Fancy: it is now high time that we should return to the plainness and soundness of observation on material and obvious things.'

In a manner almost embarrassing, Hooke apologized if he should lapse from straightforward observation. If he should make 'any small conjectures, at the causes of things . . .' he would have us construe them as 'uncertain ghesses, and not as unquestional conclusions, or matters of unconfutable science'. Consequently, he devoted himself to recording observations and adding them to the total store of knowledge—subject always to corrections from future observations. He did not want to propound hypotheses.

The task of reason is indeed difficult. There must be '. . . much rigour in admitting, much strictness in comparing, and above all much slowness in debating, and shyness in determining . . .' The Understanding 'must watch the irregularities of the senses, but it must not go before them, or prevent their information. It must examine, range, and dispose of the bank which is laid up in the memory; but it must be sure to make distinction between the sober and well collected heap, and the extravagant Idea's and mistaken images, which there it may sometimes light upon.' It is the task of philosophy [natural science] 'to begin with the hands and the eyes, and to proceed on through the memory, to be continued by the reason; nor is it to stop there, but to come about to the hands and eyes again, and so, by a continual passage round from one faculty to another, it is to be maintained in life and strength . . .' Here we have the Baconian philosophy, not only admirably expressed but put into admirable practice.

In a few places Hooke specifically mentioned medicine and physicians, and emphasized the need for recording medical experience. Experience regulated practice. By proper records the experience of thousands, extended over hundreds of years, could be made available. He noted that in recent years men had begun to record their experiences 'yet for the most part they are set down very lamely and imperfectly, and . . . to be design'd more for ostentation than for publique use. For . . . they do, for the most part, omit those experiences they have made, wherein their patients have miscarried . . . they do all along hyperbolically extol their own prescriptions, and vilifie those of others.' Nevertheless, such records, he noted, are 'generally esteemed useful'.

In closing his preface, Hooke emphasized the errors of the past, committed because mankind relied 'upon the strength of humane reason alone'. But now it was the goal of the Royal Society 'to correct all hypotheses by sense, as seamen do their dead reckoning by celestial

observations'. With the help of instruments the senses are strengthened. And by means of more accurate instrumentation we may '. . . suspect, that those effects of bodies, which have been commonly attributed to Qualities, and those confess'd to be occult, are performed by the small machines of nature, which are not to be discerned without these helps'.

This statement has great relevance to the entire methodologic change taking place. The traditional Galenists made all the observations possible with unaided senses, and then, for explanatory concepts, fell back upon Forms, Qualities, or Faculties. The Galenists passed directly from 'gross' observations to ultimate explanatory principles. However, with instruments of precision, such as the microscope, a given perception could be referred to further perceptions—to the 'small machines of nature', hitherto invisible. These perceptions through the microscope were not hypothetical. They were directly visible and they served to widen the range of explanatory reference.

If something we see with the naked eye can be 'explained' by reference to small particles visible only through the microscope, we have logical warrant for extrapolating further and invoking unseen 'submicroscopic' particles to continue the explanatory process. When we can extend the chain of explanations by microscopically perceptible objects, we have greater warrant for believing in still finer imperceptible objects which are really only hypothetical. Consequently we do not need to fall back so quickly upon the 'powers' or 'faculties' as our definitive explanations. Instruments of precision thus furnished an indirect support for the corpuscular philosophy.

Hooke was an accurate observer who generalized only with great caution and who himself followed the cautious precepts he laid out for others; but a long time would have to elapse before these precepts became truly and generally meaningful in medical theory or practice. Hooke laid out a path for medical enlightenment but physicians were slow in treading it.

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Chapter 5

Synthesis

HOFFMANN

So far in this survey I have stressed three distinct aspects. First, the metaphysical background—Aristotelian, neoplatonic, and atomistic—that, like spectacles, largely determines what we will 'see', what will stand out in sharp focus and seem important, and what will be ignored. I also indicated two readily distinguishable thought modes, corresponding roughly to the categories 'rationalism' and 'empiricism'. As examples I took Sylvius and Sydenham. Then, in the immediately preceding section I indicate differing degrees of critical acumen, as exemplified by a number of less prominent writers.

But while these different trends were revealing a transformation in medical thinking, the transformation was not always as profound as might appear on the surface. Indeed, we can draw a parallel with the conversion of the heathen, who pay lip service to new doctrines but whose own heathenish gods still have a strong hold. Many of these gods become incorporated and assimilated into the new religion, so that a transformation may be a matter more of name than of substance. Medical doctrines of the late 17th century simply adopted new names, but in many ways the substance did not change nearly as much as the names might suggest.

For the late 17th century perhaps the best single expression of the new medicine, the one which in a sense exemplifies the complex currents, is a little-known work by Friederich Hoffmann, the *Fundamenta Medicinae*, published in 1695.[1] Hoffmann was born in 1660 (the same year as Stahl and Baglivi, and eight years earlier than Boerhaave). He studied medicine at Jena and Erfurt, getting his degree in 1681 from

Jena. Later he carried out further studies in Holland and in England, where he came under the influence of Robert Boyle. After his return to the Continent, he practised with great success in Minden and won there a fine reputation. Then he went to Halberstadt in Saxony. When Frederick III, Elector of Brandenburg and later King of Prussia, founded the new university in Halle in 1693, Hoffmann was selected as professor of medicine. Stahl later became associated with Hoffmann as a second professor.

In 1695, shortly after taking up professorial duties, Hoffmann published a small textbook of medicine. The original edition was a duodecimo volume of 238 numbered pages and an important introduction of seven unnumbered pages. In Hoffmann's *Opera Omnia*[2] the work occupies 44 double-column folio pages. I have translated the book into English and the translation will be published separately.

This text had the familiar format of the *Institutes of Medicine*, with the customary five-fold division that we found in Riverius—physiology, pathology, semeiotics, hygiene, and therapeutics. From such a compendium the physician acquired basic information about the nature of the body and its mode of functioning in health; the causes of disease and the alterations produced thereby; the signs of disease whereby pathologic change could be clinically recognized and identified; the principles which governed the maintenance of health; and the medications appropriate to cure diseases.

The mode of presentation was aphoristic and dogmatic. Hoffmann did not offer the evidence on which he based his assertions, nor did he indulge in argument or justification. Short paragraphs, loosely connected, set forth doctrines quite baldly. Yet the aggregate not only provided a mass of information but revealed a philosophical position, derivative from various schools of thought, clear in some respects, confused in others. In the present chapter I wish to analyze some methodological concepts that specifically appear in Hoffmann's formulations.

If we think of medical history in its narrow sense, the *Fundamenta Medicinae* has perhaps little significance, for it did not provide any new discoveries. It did not even achieve great popularity in its own day and is rarely mentioned in medical histories. Yet from the standpoint of history of ideas it has, I believe, substantial importance. It discloses the conflict between different ideas and lets us see the way that the struggle was temporarily resolved. It shows the persistence of old ideas

under the guise of the new. It shows how some concepts were simply taken for granted while others were deemed suitable for specific exposition. It exemplifies system-formation—the integrated theory and rationalistic analysis that had such influence in the 18th century. Hoffmann's book was the precursor of Boerhaave's more famous *Institutiones* and was a logical step in the process whereby the vigorous intellectual ferment of the late 17th century became channelled into medical teaching and medical practice. Hoffmann's views led quite directly to Boerhaave and his domination of the European medical scene.

The Introduction indicated the general scheme of values: the very first paragraph praised Hippocrates and the study of nature. If we examine more closely what Hoffmann apparently intended, we find a plea for 'experience' on the one hand and natural philosophy on the other.

I have already emphasized the distinction between natural history and natural philosophy—that the one aimed primarily at description and what I have called low-level generalizations, while the other tried to offer basic and perhaps ultimate principles, which can explain the phenomena of medicine by deductive derivation. These two activities —the 'history' of nature and 'philosophy' of nature—may be opposed. Hippocrates, for the most part, we associate with description, low-level generalization and an express rejection of metaphysical principles. Hoffmann, however, wanted both the immediately descriptive and the ultimate explanatory aspects.

As Hoffmann stated in his introductory 'To the Reader', natural philosophy 'examines the hidden structures' of nature and draws conclusions. But as science it must avoid speculation. It must rest on 'solid experience' and must demonstrate and establish its conclusions by 'a simple clear mathematical method'.

Here we have the statement of rationalist dogma. Truth rests on experience—observation and sensory demonstration—and this we might even call 'natural history'. But for experience to become science it must depend on reasoning. Mathematics is the paradigm of reasoning, and mathematical demonstration is always desirable. Actually, Hoffmann did not utilize mathematics, so the term 'demonstration' must be construed in the sense of logical reasoning, supposedly as rigorous as mathematics.

Descartes was Hoffmann's ideal (he did not even mention Newton). In very large part Hoffmann adopted the mechanics of Descartes to

explain physiological processes. The current 'exalted' position of medical science, he thought, was due to mechanics 'wedded to anatomy and chemistry'. (Chemistry was understood as essentially a mechanical interplay of particles.) He adopted the mechanical philosophy entire: The [ultimate] principles of nature were matter and motion. The various forms, qualities, and powers all depended on motion acting on matter. Matter itself had different divisions, shapes, positions, and arrangements. And motion stemmed ultimately from God, the 'best mechanic and prime mover of all things'.

The physician must not only know natural philosophy but must also have wisdom and judgement in concrete situations. Hoffmann was making the distinction between the science of medicine and the art. No physician could be rational—i.e., scientific—unless well versed in natural philosophy. In addition, however, sound practical judgement in the individual case was a requisite for good medicine. 'Science is the eye of medicine, judgement and wisdom its hand.' In the so-called conflict between the ancients and the moderns, the moderns were clearly preeminent in science, but the ancients, although lacking the scientific knowledge of the moderns, did have sound practical judgement.

In his Introduction, Hoffmann revealed his own view of his own procedure. 'I have provided what is true, what can be demonstrated and is supported by the principles of physics and mechanics.' [The word 'demonstrated' has a double meaning. One refers to empirical observation, the other to mathematical or logical inference.] Hoffmann wanted to set out the firm basic principles of medicine and then reduce these into a brief system, according to the precepts of the new philosophy. In this way the whole science of medicine could 'be properly acquired in a short time'.

Let us see how he actually implemented his intentions.

II

The scientific physician must be able to answer the question, 'Why?' or 'How?' He must know the reasons for things. He must give rational explanations, for otherwise he would be only an empiric.

The term 'explanation' has innumerable shades of meaning that have

received attention from philosophers. In the present context I would indicate only some interpretations significant in 17th-century medicine. In this context, by 'explanation' I mean simply the process whereby some particular observation or phenomenon is *referred* to something else. The reference may be to something concrete, in which case we call it an analogy (e.g., 'explaining' the action of the lungs by comparing it to a bellows). In this sense the phenomenon is connected up with something quite familiar, and thereby gets better understood. Or, the phenomenon in question might be referred to some hypothetical entity (to take a modern example, the conceptual 'gremlin' that served to 'explain' so many odd happenings during World War II). Or the reference may be to some concept or general principle abstracted from experience. The individual phenomenon may then be subsumed under a 'generalization' (e.g., 'explaining' the action of a spring by invoking the principles of elasticity). This is a 'rational' explanation. Given the concept or principle, the particular phenomenon can be derived by deduction therefrom, and the particular phenomenon thus becomes part of a logical and rational system where the principle is *logically* prior to the concrete phenomena.

The concepts, generalizations from experience, or 'laws', can exist in increasing degrees of abstraction, with corresponding increased generality. The rationalist tries to find the concepts of maximum generality which could explain *all* phenomena. Descartes serves as the classical example, for he enunciated two ultimate concepts, extension and motion, that could by appropriate inference explain all natural phenomena.

We can think of logical explanation as a pyramid, with the ultimate abstractions at the apex and the concrete observations at the base. A whole series of step-wise intermediaries may exist between the apex and the base. Any given level can 'explain' the levels below it, and 'is explained' by higher levels. Near the bottom are relatively 'low-level' generalizations, close to experience and 'logically' below the more abstract concepts. Francis Bacon, and the empiricists, decried the tendency to go directly from concrete experience to high level abstractions, but felt that progress should occur through gradual ascent—through concepts and principles of slowly increasing generality.

Hoffmann was a rationalist who wished to systematize the concepts involved in medicine. He did not, unfortunately, provide an orderly arrangement but from his text we can derive his explanatory principles, in their varying degrees of abstraction.

In the 17th century, as we have already discussed in preceding chapters, there were three main metaphysical formulations or sets of first principles. One was the Aristotelian, with forms and matter; the second was largely neoplatonic, as exemplified in van Helmont, with undifferentiated water and specific differentiating ferments; and the third we may call the corpuscular view, with Descartes and Gassendi as somewhat divergent proponents. All of these presented different ultimate concepts as the apex of the explanatory pyramids or the last link in the chains of explanation. But for practical usefulness, meta-physical abstraction must connect up with concrete observed data through more 'proximal' links.

III

I want to describe some of these explanatory concepts that Hoffmann proposed, concepts that he integrated into a system. For expository purposes I consider separately the phenomena of health or normal physiological mechanisms and the phenomena of disease.

Hoffmann followed the corpuscular philosophy whose first principles were matter and motion. He expressly denied the Aristotelian forms and primary qualities, as well as the teachings of the chemists. Follow-ing Descartes, he considered the body to be a machine. There was, in addition, the mind and soul, and he faced the problems of the nature of mind and the relations of mind and body.

The body was composed of fluids and solids, with the fluids holding the important position. The principal fluids were the blood and serum, the lymph, and the animal spirits. The blood, serum, and lymph were grossly observable; the animal spirits were inferential only. No one could perceive animal spirits, either by naked eye or microscopic obser-vation, yet physicians attributed to animal spirits, which they could not see, all the reality of the blood which they could see.

However, when Hoffmann referred any phenomenon to 'blood' or 'lymph', he did not necessarily mean the grossly observable properties of these fluids, but rather certain hypothetical aspects, namely, the constituent corpuscles. Hoffmann constantly spoke about the particles or corpuscles of the blood. By these he did not, of course, have in mind 'corpuscles' in the modern sense of red blood cells or white blood cells.

He meant it in the literal sense of 'little bodies', so little that they could not be seen. But their presence could be inferred, as well as their characteristics.

The blood and lymph were composed, he thought, of many different elemental particles whose characteristics derived from the rather confused chemistry of the day. He identified the different particles by certain qualities—earthy, watery, saline, volatile, fixed, branching, sulphurous, etc.,[3] but he made no attempt to offer any exhaustive listing. Direct observation was not involved but all the particles were inferential only. Many of his chemical ideas he clearly derived from Sylvius, whose views are discussed in a previous chapter.

To avoid Aristotelian taint, Hoffmann tried to make sensible qualities depend on particulate corpuscles. Various particles, somehow, provided specific qualities or properties. Some of these could be perceived more or less directly: the property 'volatility' could be readily observed—some fluids evaporated readily; so too with 'acidity' —some fluids were sour to the taste (or induced appropriate colour changes in vegetable extracts). Other entities, like the 'branching' (*ramosa*) corpuscles, did not represent discrete qualities but were purely inferential, with an even higher degree of abstraction. But how the corpuscles might connect up with a quality or property he did not try to say.

Thus, the properties of the blood could 'explain' a great deal, but the explanatory force came from the hypothetical parts which we could not see. To illustrate the explanatory process we may, for example, refer to the different 'temperaments'. These differences were facts of observation; some people, for example, were sluggish, others vigorous, active, excitable. These temperaments had long been attributed to different proportions of the humours. Galenic writers invoked a preponderance of one or another of the primary qualities. But Hoffmann invoked the particulate components in some characteristic proportion. Thus, 'when sulphurous, oily, volatile or saline particles predominate over the fixed or watery, or earthy ones, the disproportion is *warm*, and then the resulting temperament is called warm or sanguine; and if the excess is too great, then a choleric temperament results.'[4]

Here we have three features of significance. First, the Aristotelian qualities—warm or cold, moist or dry—were recognized as adequately descriptive and were preserved. Second, the qualities, dependent on the number, configuration, character and proportion of various corpuscles,

were thus reduced from *primary* to *secondary* status. Third, Hoffmann emphasized the quantitative aspect of *proportion* as an explanatory principle.

Here we have a fine blend of the old and the new. The idea of 'proportion' was, of course, deeply embedded in Greek thought and has influenced all medicine from Hippocrates to the present. The term exudes a sort of quantitative aura, as if some sort of *formula* were at work in the world. The actual components to which the formula applied, whether immaterial or material, abstract or concrete, did not matter too much. If only the components conformed to the formula, then all would be well and we would have a 'rightness'. If the components were not harmonious, then something was wrong. Implied here, of course, was a sense of normality, of certain broad patterns of health, recognized from observation of nature. And also implied was the correlation between particular patterns and a particular distribution of the component entities.

The Greek philosophers who concerned themselves with medicine recognized that if the proportions varied, the state of health would vary, but there was little attempt to express the proportion in precise terms. In general, the use of 'proportion' and 'measure' had no specificity beyond the indefinite terms 'appropriate' or 'suitable'. Hoffmann declared, 'If the fluids are constituted in a suitable proportion . . . and the solids exist in a suitable configuration, position, and order, the body . . . is healthy'.[5] In regard to precision he did not improve much on Hippocrates.

Hoffman, following the 17th century corpuscularians, applied the concept of 'suitable' proportion to hypothetical particles. If temperaments, e.g., were defined in terms of particles rather than humours or qualities, the possibility existed for eventual integration with other and more exact branches of science. The corpuscular theory opened the door for possible formulation, eventually, in a specific quantitative fashion. Paying attention to corpuscles, more or less alike, fostered this development in a way that attention to substantial forms did not.

In the 17th century an explanation of phenomena by corpuscles did not have any concrete advantage over explanation by Aristotelian forms and Galenic faculties. There was, however, a *potential* link with further progress. In retrospect we can see that the link was in fact eventually forged, but *at the time* any belief in future progress depended principally on faith.

Among Hoffman's explanatory concepts motion held first place. Harvey's great discovery of the circulation had brought awareness that the uninterrupted motion of the blood was requisite for health and life. The circulatory motion of the blood, through arteries, capillaries, and veins, was an accepted 'fact', which could be observed, more or less directly. But this type of motion, a necessity for life, had only limited explanatory value for particular biological phenomena. To account for specific physiological or pathological events, Hoffmann relied on a hypothetical 'internal' motion that involved the hypothetical constituent blood particles. This internal motion was itself inferential and Hoffmann attributed to it various functional properties such as speed and direction. Changes in speed and internal motion would explain particular phenomena, as we shall see especially in pathology.

Further explanatory principles came from hydrodynamic concepts that Descartes had done so much to popularize. Descartes had, in a theoretical way, stressed the relationship between animal physiology and the passage of fluids through various tubes. His analogies, even though in large part published posthumously, were well known. If we invoke tubes of different diameters, with fluid flowing at different rates, we have many additional variables which might serve as explanatory correlates. Hoffmann was not at all disturbed that all of these concepts were entirely hypothetical.

Particularly significant was the concept of the animal spirits, the primary motive power of the body. The animal spirits which induced movement in the body had certain relationships to the source of cosmic motion, i.e., to the metaphysical substance that induced movement in the entire universe. Hoffmann relied largely on the Cartesian metaphysics. Descartes had distinguished three kinds of matter, three different subdivisions of 'extension'. These differed in size of particles.[6] The very finest, ' extremely subtle matter', was that which filled up all the interstices of the Cartesian plenum. This finest or extremely subtle matter was the first cause of motion and stemmed ultimately from God.[7]

If we regard not the universe as a whole but rather the living body, we find the animal spirits as the finest of all humours. Hoffmann designated the animal spirits as 'a sort of prime mover'[8] [*quasi impetum facientes*]. What are the relations between the animal spirits in the body and the extremely subtle metaphysical matter of Descartes? And between animal spirits, mind and soul? Hoffmann was not very explicit on these points but we can make certain inferences from what he did say.

He declared that in the body the first principle of motion was the soul [Latin *anima*, which we can render as either 'soul' or 'vital principle']. This, he said, we may consider as 'nature, or spirit endowed with mechanical powers, or a most subtle ethereal matter acting in an ordered and specific fashion'.[9] I believe that this represents neoplatonism. To deny the world of pure and immaterial spirit, the realm of God, was unthinkable. In neoplatonism the immaterial spirit transformed into extremely subtle matter, extremely fine but nevertheless somewhat material, and this in turn acted on more crude matter. This was part of the gradual transformation from the immaterial world of divine spirit, through successive emanations, to crude matter. The Cartesian extremely subtle matter shows manifest similarities to the older neoplatonic equivalent, with the difference that Descartes invoked a specific act of divine creation, while neoplatonism relied on the principle of emanation.

Hoffmann needed a connecting link between the immaterial pure spirit and crude matter. To bridge the gap he, like the neoplatonists, postulated a substance that shared in the properties of both realms—a transitional substance, a spirit which was no longer 'pure' but ever so slightly material.

The vital principle or *anima* which man shared with the brutes was thus a finely material substance, but nevertheless distinct from the animal spirits. The *anima* used the animal spirits as an instrument for carrying out appropriate functions. The *anima* moved the animal spirits which in turn moved the muscles or fibres.[10] There is thus a chain of activity with progressively increasing materiality in the different links.

The vital spirits as a physiological entity, as the substance that flowed through nerves, was a humour finer than any other, but otherwise comparable. It was definitely a physical substance which derived ultimately from the ingested food and was produced in the brain. The *anima*, still finer, had both a fine materiality and yet a metaphysical dimension. In man and animals this vital principle had the power of initiating movement; but in man, as distinct from animals, it was also provided [*instructam*] with a more noble power [*nobiliori potentia*],[11] namely, the power of thinking and reasoning. This power was called 'mind' [*mens*] and was an immaterial substance created by God himself.

In this way Hoffmann preserved both theological dogma and natural philosophy. Descartes had created a sharp dualism of mind and matter. Hoffmann, utilizing some of the Cartesian ideas, tried to minimize the

dualism and harmonize all aspects of existence. He did this through what I would consider a neoplatonic manoeuvre. Nature and life were readily explicable by a hierarchy of Being. Man, as merely a living creature, possessed the attributes of nature. But his mind, unique and entirely immaterial, came directly from God and fastened in, somehow, to the *anima*. We thus have, for man, a complex system that included mind, *anima*, and animal spirits. In the [immaterial] mind we have [immaterial] ideas which somehow get impressed on the finely material intermediary, and thus influence the more coarsely material substances of body.

The animal spirits had a 'power' of bringing about purposeful movements toward a definite goal. This power, inherent in the animal spirits, derived from God. Hoffmann called it the 'sensitive soul', [*anima sensitiva*] and it existed only in the fluid of the brain.[12] 'Ideas' can get impressed on the animal spirits and the impressions can last for a long time.[13] This represented Hoffmann's explanation for the concrete observation that the body can acquire certain periodic physiological 'habits', e.g., regularity in excretion. There seemed to be an interplay between conscious intent—an idea—and eventual automatic performance. To explain the phenomenon Hoffmann assumed that an 'idea' could get 'impressed' on the animal spirits that triggered the muscles. The animal spirits thus acquired a 'power'. But this, of course, is only a transmutation of Galenic 'faculty' into a vocabulary of matter and motion. Hoffmann provided not any new insight but rather a translation from one vocabulary into another. In this connection reference may be made to a previous study[14] on animism and the relation between Hoffmann and Stahl—a relationship that had special significance after the turn of the 18th century. Hoffmann, in essence, took a sort of animistic view, but by verbal juggling *called* it mechanical.

The animal spirits mediated nerve functions. In physiology of the sense organs, for example, the sensed object [in whatever sensory modality] transferred its impact to the animal spirits which in turn transferred the motion to the common sensory, where perception took place. All sensation was a form of touch, and differences in perception resulted from different impressions or motions. The various sensory qualities, of course, were not in the object but only in the mind.

The motions constituting sensation arrived at the common sensory which Hoffmann placed in the *centrum ovale*—'the highest place for guiding the will and decisions of the mind'.[15] The *centrum ovale* took

the place of Descartes' pineal gland. Hoffmann recognized the differences between voluntary and involuntary motion. The latter took place in the cerebellum, the former in the cerebrum. He also had appropriate mechanical explanations for memory and imagination.

Hoffmann covered briefly the functions of various physiological systems—circulation, respiration, generation, glandular secretion, digestion, and so on. The translation, to be published, will give details. The solid parts of the body depended on the fibres, and the differentiation of various parts was a function of size, delicacy, position, and compactness of fibres.[16] And fibres formed tubules of various sorts. However, the *Fundamenta* exhibited only a modest interest in structural or anatomical details.

Metabolic functions depended on internal motion of different types, affecting different kinds of particles. The proper functioning of particular organs depended on the proper influx of animal spirits which, as the *deus ex machina*, explained all observed functions in terms consistent with the mechanical philosophy.

Motion of the blood had great significance, especially the hypothesized internal motion, on which 'depends the formation of spirits, strength, and energy itself'. Important, too, was the 'elastic air'. 'The motion of the blood is maintained by subtle particles of elastic air which, by virtue of shape and size, bring about a certain vital swelling in various particles of the blood. On this swelling depend the energy of the body and at the same time the motion of the heart itself.' Again, 'the more subtle portion of the air, endowed with elastic force, divides the globules of the blood and drives them apart, produces motion or internal fermentation, fluidity, vital heat, and a purplish colour'[17].

Heat resulted from the violent internal motion of the very fine particles of the blood.[18] Heat fed on the fluid parts and liquefied the solids. The greater the heat the more the parts were consumed.

Hoffmann would not, of course, allow in his medical system any concept like the Archeus of van Helmont. He would not permit any *natural* functions of the body, anything physiological, to be controlled by any agent that was personalized, that might exert choice or 'voluntary' activity. Only the mind of man had free will. For all bodily functions Hoffmann was committed to a mechanical explanation that involved particles in motion. And yet, some of the bodily activities seemed to exhibit a definite order and pattern. To explain, e.g., the exquisite selectivity wherein each organ was specifically nourished by

the appropriate nutrient particles, Hoffmann expressly rejected any 'faculty'. He also rejected any specific 'ferment', as well as any particular arrangement or configuration of pores which might have provided a simple 'mechanical' explanation. Instead, he invoked a 'movement' of the 'sensitive soul, determined to a definite order, measure, and number; in a word, a plastic force [*vis plastica*]'.[19]

Hoffmann was entirely unable to eliminate Aristotelian thinking. He could not escape the concept of 'ideas' being impressed on a suitable substrate—a relic of Forms getting impressed on Matter. We see this, for example, in his explanation of menstruation: 'Nature, by a certain Form impressed upon it, and by a matter of habit, gives off the superfluous juice through the uterus'.[20] When he could not explain phenomena by referring them to known rules of mechanics, he attributed the activity to an 'idea' impressed upon nature and called it a plastic force.

The willingness to invoke a 'plastic force' to explain phenomena, while at the same time rejecting the faculties, ferments, animism, or crude mechanism, represented an act of faith. Unable to arrive at even hypothetical details of a mechanism, he took refuge in the words 'plastic force' and arbitrarily placed this entity in the vocabulary of mechanical philosophy. As I have indicated elsewhere,[21] Hoffmann's views were remarkably similar to Stahl's but rested on a different metaphysics, supported by faith rather than evidence.

In his physiology Hoffmann explained all phenomena by particles having essentially hypothetical qualities and undergoing hypothetical motion, combined in hypothetical proportions, under the control of a special kind of hypothetical force, that acted largely through hypothetical animal spirits, controlled by a hypothetical *anima*, with which, in man, was associated a conscious mind. His explanations rested on abstractions that did have some basis in experience and that he thought derived logically from concrete observation. The essence of 17th century medical thought consists perhaps in this: that Hoffmann perceived no inconsistency between his expressed methodological ideals and the views he actually propounded. Progress of medicine, in the subsequent century and a half, required that a gradual awareness of incongruity should develop. And eventually it did develop, but only after many vicissitudes.

IV

When the mechanisms of health get distorted, disease may arise. At first glance this formulation might seem a mere tautology, considering health as the absence of disease and disease as the absence of health. But such a judgment is not quite correct. Health requires the interrelation of many separate factors and a certain quantitative ratio may serve as a meaningful determinant.

To express this idea of quantitativeness Hoffmann used the Latin word *temperies*—the due and suitable proportion, a concept, as noted earlier, deriving from Hippocrates, Aristotle, and Galen, and maintaining its significance well into the 18th century. What *was* the actual proportion that was suitable and proper? *How much* of this or that component was required? Hoffmann did not tell us. He only indicated that in disease the 'proper' became 'improper'; *temperies* gave way to *intemperies*. No techniques were available to provide quantitative measure. Physicians could not go beyond the rough estimation 'too much', 'just right', and 'too little'. 'Too much' and 'too little' constituted disease, 'just right', health. I would emphasize that this sort of division did represent a rough—very rough—quantitative analysis. And it set up the framework for eventual numerical determination.

We naturally ask, too much or too little *what*? What components were being subjected to this crude estimation? The Galenists, as we have seen, fell back on the humours, the qualities, and faculties. Hoffmann, as a corpuscularian, had a different basis, which he expounded in the first section of his book, the part devoted to physiology.

His formulations of disease, however, involved several levels of abstraction. In part he remained quite close to concrete experience, and for his explanation utilized directly observable substances such as the humours. For example, *plethora*, as an over-abundance of blood, might be more or less directly observed. On the other hand, the over-abundance might refer to quite hypothetical substances assumed to exist in the blood but not directly observed. A plethora of blood was close to concrete experience. An excess of acid or earthy particles was not.

Among the particles involved in disease, we must distinguish between those normally present in the body—the kind that he mentioned in his

physiology—and foreign substances which caused disease by entering the body from outside. These included salts and ferments, and they had considerable specificity in 'causing' disease.

In regard to motion we also must distinguish two types. One was the circulatory motion of the blood as well as of the lymph. Health demanded an intact circulation. Interference with it—by whatever means—could induce disease. Obstruction of a blood vessel was perhaps the most significant mode of impairing the circulatory motion of the blood. But as I have already noted, Hoffmann also invoked a quite different type of motion, purely inferential and far removed from direct observation, namely, the so-called internal motion of the blood, that involved the invisible particles in the blood. Theory demanded that the particles be in motion. Therefore they were assumed to be moving, and the assumption achieved the status of a dogma, accepted without direct evidence.

Kargon has well expressed the underlying point of view when he discussed the great physicist-theorists of the 1650's—Descartes, Hobbes, and Gassendi. These men 'all realized that in dealing with the motions of invisible and undetectable particles they were going far beyond the bounds of certitude. For them, therefore, the study of physics consisted in delineating possible opinions, limited only by two considerations: these opinions were not to conflict with any experience, nor should they lead logically to any absurdity once the primary hypotheses were granted.'[22] This viewpoint of theoretical physics also applied to Hoffmann's doctrines. He had to avoid inconsistency with observed fact or with other theoretical considerations. Because he did avoid these pitfalls, he was quite confident that his claims were valid: they represented inferences derived from basic theory, and they did not contradict any known evidence. To be sure, they had no direct positive evidence in their favour, but this was not important under the prevailing scheme of values. The enlightenment in medicine was not fully completed until a new scheme of values took over, and physicians recognized the need for positive evidence.

Meanwhile, for Hoffmann the concept of internal motion remained a potent tool, capable of explaining many phenomena, including metabolic processes and body heat. Thus, 'since the natural heat is due to the internal motion of the ethereal particles in the pores and interstices of the blood, a praeternatural and stronger heat arises from the violent and very rapid passage of the ether through these same

pores'.[23] Whether these were *good* explanations is quite another question.

The views so far presented have to do principally with the humours. However, Hoffmann did not ignore the solids, even though he gave them a subordinate role. Some disorders of the solids were quite obvious —traumata such as wounds or dislocations or fractures; or malformations. More subtle were, for example, disturbances of the blood vessels. A fracture would be grossly observable; the blockage of a blood vessel might be highly inferential. Indeed, the blood vessel itself as well as its blockage might be equally hypothetical. In general the disturbances of the solids had significance proportional to the degree that they interfered with the function of the humours.

Certain other concepts appeared in Hoffmann's explanatory schemata. From time to time he spoke of 'corruption', although we cannot be quite sure just what he comprehended by this term. In one paragraph he applied it to the humours, grossly observable: under certain conditions 'the humours not only stagnate but become corrupt'.[24] But in the very next paragraph he spoke of 'corrupt particles, gathered in damaged pores'. I believe that Hoffmann was maintaining the tradition of the 'ancients', and referred by choice to the humours and their changes in quantity and quality. But he also held to the 'modern' viewpoint, which required explanation by particulate matter. He was forced, therefore, to adapt corpuscular terminology to traditional concepts, and the result was a rather confused mixture that will not bear rigorous examination. We shall see other examples of this confusion between Aristotelian (and sometimes neoplatonic) background and the corpuscular nomenclature.

We may at this point consider Hoffmann's discussion of some specific diseases. At the time that Hoffmann wrote, nosology was quite undeveloped and there was no adequate classification of diseases. Nevertheless, he did distinguish some approximate categories, and from these I would select a few specific examples.

Intermittent fevers, a major category, resulted from a 'febrile material'. 'Transmutation' of fevers occurred as the 'shape, mass, and motion of the morbific material are altered and changed'.[25] Hoffmann tried to draw a rough correlation between the various kinds of intermittent fevers—quotidian, tertian, or quartan—and the degree to

which the particles were sharp and irritating. The more active and sharper the morbific material, the more quickly did it irritate the fibres, force them to spasmodic contraction, and in this way bring on a paroxysm.[26]

Quinine was a 'specific' remedy for intermittent fevers, since it corrected the 'defective quality' of the febrile material. Quinine did this by its 'bitter, earthy, gently astringent and resinous-balsamic particles'. These not only acted on the febrile material but corrected the texture and 'crasis' of the blood and strengthened the pores and sub-cutaneous vessels that were too lax and too disposed to stasis.[27] He thus implicated, in part, a faulty publication.

In contrast to the intermittent fevers were the continued fevers. As an example of these we can take smallpox, whose specificity was well recognized. The agent in smallpox was a 'caustic salt, of a particular sharp type'. This specific salt arose from impure particles of an 'acid nutriment' closely bound to the blood. The corrupt particles became *released* under certain conditions as, under the influx of some particular ethereal matter or air [*aethere ac aëre influente*]. When this occurred, the caustic particles were separated from those which had served to blunt and conceal them—a sort of chelation—and became active. This caustic salt then set the heart and arteries into a more violent motion, disturbed the crasis of the blood and produced fever.[28]

Hoffmann had an interesting hydrodynamic explanation for the way the cutaneous lesions came about. In flowing streams which have an 'eddying' motion, foreign material is thrown to the periphery, just as a flowing river drives pieces of wood to the banks. Similarly, with particles in the blood stream. If, driven to the periphery, the foreign material did not then find suitable pores by which it could be eliminated, it stagnated and produced pustules and ulcers.[29] He did not explain what made the pores 'suitable'.

Hoffmann made very clear that the morbific material could remain in the blood a long time, bound and rendered harmless, until it got separated from the bond. Violent physical and mental activities, and a 'particular disposition of the air', might release the contagious salt entangled in the blood and set it into operation.[30]

As his explanatory principles, therefore, Hoffmann has taken some concepts from chemistry and others from hydrodynamics. In addition, he has used some of the old Greek concepts of impure air—one of the six non-naturals—as an environmental factor. He did not specify or

characterize the particular salt that caused smallpox except to call it sharp and caustic—a description more of its effects than its intrinsic nature. In contrast, he believed that the causative factor in measles was also a 'certain specific caustic salt' but obviously not nearly so caustic as that of smallpox. The salt of measles, he believed, derived from some property of the milk, taken in at the time of lactation, and remaining in the blood until release.[31]

Hoffmann distinguished three kinds of poisons. Some were corrosive and acted directly on the solid parts by corroding and inflaming. Others, vaporous, filled the minute nerve tubules and impaired the animal spirits (e.g., opium and narcotics). Still others, 'fermentative', produced a great 'ebullition' and infected the animal spirits with a sharp and motile volatile salt. Exemplifying this latter type was the infectious material in the saliva of a rabid animal, transferred by a bite. Such a poison, he said, contained a precise *form* of determinate motion [*exactam motus determinati ideam*]. I translate the Latin *idea* by the English word *form*. In accordance with this form or idea the poison multiplied in the animal spirits, destroyed their regular motion and brought about a motion similar to that of the form itself [*similem idealem motum efficiendo*].[32]

Here, Hoffmann described a ferment acting on a suitable substrate. A ferment required some material suitable for the fermentation, and 'similarly' a contagious poison would not act unless the blood was suitable for receiving its motion.[33] In contagious malignant disease, as in rabies, the contagious material consisted of fine effluvia given off from the diseased body. These particles 'disposed' the blood to take on their 'own nature' and also introduced a 'motion similar to their own'.[34] The spirits and the blood of the infected person thus came to assume the particular nature and the particular motion imparted by the effluvia.

Like other physicians Hoffmann had been impressed by the extremely minute quantity of infectious material that could induce rabies. The mode of action was generally similar to a ferment such as we find in the manufacture of beer, but the characteristics of rabies ferment were quite different from any known chemical substance. A 'salt' might act by some familiar property such as sharpness, and if this were postulated, could erode by simple mechanical effect. But if the disease rabies were due to minute particles, these showed specific modes of activity quite different from the ordinary phenomena of mechanics. To explain the pattern of phenomena Hoffmann fell back on *idea*, impressed upon the

particles. The *idea* involved a specific characteristic quality with a definite temporal course and a specific pattern of behaviour. Such a quality is quite comparable to the Aristotelian *form* that conveyed specificity to material entities. And for this reason I believe that in this context *idea* requires a translation *form*.

In some usages he indicated that *idea* meant a special sort of motion. Referring, for example, to the animal spirits he declared that 'a particular idea of motion' could be impressed upon them. The spirits would firmly retain this *idea* and 'then bring about movements ordered in accordance with this idea'.[35] He did not really harmonize the *idea* with the corpuscular philosophy. He merely joined the two together.

Disturbance of circulatory motion caused many diseases. Especially important for our purposes was the local stagnation of humours that could result in impaired circulation. Such stagnation could produce tumours, inflammations, ulcers, and suppression of secretions. Inflammations might result from some 'irregular destructive salts having an angular configuration'[36] that stimulated the fibres, contracted them, and narrowed the vessels, thus further hindering the circulation.

The influence of Sylvius is particularly strong here. When humours stagnated they might 'become sharp', get 'corrupted', and 'turn into pus'.[37] This was the process of corruption which Hoffmann did not analyse directly, but it seems to be related to fermentation. In one place, relative to pus in an ulcer, he spoke of the juices being corrupted by an 'acid ferment'[38]; but the whole subject of pus, despite its importance, he did not examine in detail.

He blamed cancer on an obstruction, but one involving the lymph channels rather than the blood vessels. If stagnation occurred in a narrow lymph channel, coagulation might take place. This could not be converted into pus but would harden and become a 'scirrhus'. Then, if a 'corrosive acid substance' were involved, a cancer could occur.[39] Stagnation without any corrosive action would produce other sorts of swellings.

Physicians could readily recognize various distinct disease entities —'intermittent fevers', smallpox, measles, rabies, various inflammations, cancers, 'leucophlegmasias', and so on. From the clinical standpoint these could all be quite clearly differentiated one from the other. To explain them Hoffmann had at his disposal only a limited number of terms, not enough to go around. Hence he had to explain quite

different diseases by essentially the same concepts. All sorts of inflammatory conditions he attributed to salts of varying types, having various destructive or corrosive actions. The whole concept of 'acrimony', as developed by Sylvius, was emphasized in Hoffmann's writings. He assumed that certain diseases resulted from the action of salts, and then he assumed that the salts must have some sort of differences one from the other, thus piling hypothesis upon hypothesis.

A single condition such as 'poylpoid concretion', which acted by disturbing the course of the blood, could, he declared, 'very easily cause dyspnea, peripneumonia, suffocating catarrh, ruptures of the vessels, or haemoptysis'.[40] But he did not give any satisfactory reason why the disturbance should in the one case cause dyspnea and in another haemoptysis. Or, impaired circulation through very delicate canals might produce 'sudamina, vesicles, the itch mites, various pustules, acne rosacea, and so forth'.[41] He could invoke a greater or lesser degree of 'acrimony' to differentiate the causative factors. He did not offer specific differentiating features.

Some help, of course, could come from environmental factors. These, particularly as the so-called non-naturals, could serve as 'remote causes' of disease. The importance of air, food, or water, for example, had long been recognized, and from the time of Hippocratic medicine, at least, has been correlated with diseases. Such correlations, of course, were on a relatively concrete and proximal level. Data from these factors or from the other non-naturals such as the excretions, sleep and wakefulness, exercise, the emotions, could readily connect up with many disease states. They might help to differentiate the pathogenesis of various diseases.

It would be easy to condemn Hoffmann for relying on 'unverified hypotheses' but such criticisms, I believe, would not be justified; they would be rejecting the context of the era and applying 19th- and 20th-century values to 17th-century thought. It is true that Hoffmann failed to verify his hypotheses, but this is not very significant. Far more significantly, *he did not perceive any need for concrete evidence*. He asserted some generalizations derived from contemporary science, he made some logical derivations, asserted a relationship to empirical events, and then stopped. *The logical derivations and implications he thought were cogent*. The reliance on rationalism, inference, and assertion formed the core of his methodology.

V

In the gradual change-over from Galenism to the mechanistic view of medicine, one important propelling factor was the sheer volume of new data. New *facts*, new 'bits of information', were rapidly accumulating, in physics and chemistry, anatomy, physiology, and therapeutics. These new data required organization and assimilation into an orderly framework. The old Aristotelian framework of form and matter, qualities and faculties, found the new information rather difficult to handle. The atomic or corpuscularian theories were an easier way to make all the new data cohere among themselves and harmonize with older accepted data. The conflict between two explanatory systems we have seen clearly delineated in Robert Boyle, and we have seen how well he marshalled the corpuscularian forces to provide an acceptable widely-embracing theory.

The second half of the 17th century saw what I would call an aesthetic awareness of theory: we have a lot of facts to be ordered and explained; we have competing theories to perform the explanation; which theory is *better*? Which carries out the task of systematization in a more effective and comprehensive fashion? The answer depends on an awareness that I can only call aesthetic—a sense of fitness, or elegance, or comprehensiveness. Either theory *could* explain all the data. Faculties and qualities and substantial forms had indeed satisfied critical minds for a long time. But the satisfaction diminished, and competing theory became more attractive.

There were no simple grounds for decision between them, no 'crucial experiment' which 'proved' one or 'disproved' the other. Such terms are quite repugnant to the historian of medical ideas. Neither the Aristotelian nor the corpuscularian theories were capable of 'proof' in medicine. Physicians did not agree on what constituted facts, what allegations were true or false. Medicine was not a whole body of information that could be subjected to tests. Instead, one theory could explain certain data and would ignore others; proponents of that theory would consider the ignored data quite unimportant. Proponents of an opposite theory would consider different facets important and ignore still others. In a sense, the latter 17th century was a conflict over the topic, What will you ignore? What will you consider irrelevant?

Yet the corpuscularian views, on the whole, became more and more successful as an explanatory tool. The awareness among physicians that somehow it was 'better', at least for certain aspects, marks an important step in the development of critical acumen. There was an implicit evaluation, the first and perhaps the most important step in the growth of critical awareness.

In the latter 17th century medical theories faced certain tasks. One had to do with specificity. There were many different diseases, each quite specific. What could account for the specificity? How satisfying would the explanation be? How consistent with other data? Forms and faculties were perfectly good explanations so long as they were deemed to be real entities. But if they had only a secondary or derivative existence, they became mere verbalisms and not at all satisfying. What *real* entities could correlate with specific diseases? with specific remedies? The answer, obviously, depended on the views of Reality.

A second problem had to do with aspects of reality that were immaterial but nevertheless real. God, soul, and mind were all considered real, even by the corpuscularians. God could be separated off from the domain of science and shunted to one side. And similarly with soul as a theological concept. But animals, that lacked any theological soul, nevertheless possessed a principle of life. Was this principle of life material or immaterial? Was it a pattern of atoms? Or an immaterial entity in its own right? Or an entity partly material, partly immaterial, a special kind of matter? These problems, closely connected with the contemporary thinking, could be ignored by some, but for others assumed an important place. Furthermore, the place of 'mind' and ideas, perception, intellection, will, all that we today would call 'psychology', had a place in the medical scene, a place important for some physicians, not important for others.

Somewhat related was the concept of 'guiding' forces acting in living organisms. Aristotle had spoken of 'entelechy' and Galen of 'faculty'. Van Helmont had 'Archei' of varying kinds. Hoffmann refused to accord any reality to such concepts, but he himself spoke of a 'plastic' principle or power. Was the body a machine? If so, how to account for the behaviour and activities that man-made machines did not exhibit? Or were these problems unimportant?

The *Fundamenta Medicinae* is not as detailed as we might like nor as analytical. But its very imperfections and inconsistencies reveal the thought processes of the time and the seething intellectual activity of

the era. In the present discussion I indicate only a few of the problems that emerge from the book, problems that have to do particularly with methodology, basic attitudes, and relationships to previous workers. These aspects have particular importance in studying the gradual transformation of medicine.

In place of the Galenic terminology of Riverius, Hoffmann has adopted a new vocabulary, with terms drawn from mechanics and hydrodynamics, physiology, chemistry, anatomy. But once we make allowances for the altered terminology, we see that the outlook, the dogmatism, the lack of critical examination are surprisingly similar. Hoffmann pays explicit homage to the twin goddesses of experience and reason, but the goddesses themselves were old Galenic friends, and Hoffmann's liturgy did not differ very much from the earlier forms of worship.

To be sure, a new spirit of change was in the air, apparent in many individual scientists. There was a great deal of curiosity and a great deal of experimentation, and with the experimentations a substantial critical acuity. But the critical acumen had spread only a little way into clinical medicine, and had not yet affected the basic rationalism of various medical systems.

Systems, with changing content, would dominate the entire 18th century and even much of the 19th, but a new relationship was being forged between experience and reason. And the new relationship, in a sense, reflects the growing critical awareness that constitutes the true medical enlightenment.

The end of the 17th century concludes what we may call a prolegomenon of modern medicine. A new vocabulary has been introduced. New systems are being formed, and will lead to a new dogmatism in the first part of the 18th century. But the new dogmatism will not stand against the progressive inroads of empirical investigation and critical attitude. These changes bring about an enlightenment to which the 17th century was the preliminary approach.

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